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THE RELATION OF PHYSICAL CHEMISTRY TO PHYSICS AND CHEMISTRY.[1]

ACCORDING to the program, I have to consider the 'General Principles and Fundamental Conceptions which connect Physical Chemistry with the Related Sciences, reviewing in this Way the Development of the Science in Question itself.'

Let me begin by defining physical chemistry as the science devoted to the introduction of physical knowledge into chemistry, with the aim of being useful to the latter. On this basis I can limit my task to the relations of physical chemistry to the two sciences it unites, chemistry and physics.

But even if I limit myself to these relations, which are not the only two,[2] I wish to restrict myself yet more, in order, in the spirit of this congress, to call your attention to broad views. So I shall follow up only two lines, in answering two questions regarding two fundamental problems in chemistry: (1) What has physical chemistry done for our ideas concerning matter? (2) What has it done for our ideas concerning affinity?

The small table which I have the honor to put before you will enable us to answer these questions by appeal to the scientific development of our science, which also I have to review:

I. IDEAS CONCERNING MATTER.

1. Lavoisier, Dalton (1808).
2. Gay-Lussac, Avogadro (1811).

[1] Read before the International Congress of Arts and Science.

[2] In Chicago I devoted to this subject eight lectures, which have since appeared in the Decennial Publications under the title 'Physical Chemistry in the Service of the Sciences,' Chicago, 1903.

3. Dulong, Petit, Mitscherlich (1820).
4. Faraday (1832).
5. Bunsen, Kirchhoff (1861).
6. Periodic System (1869).
7. Pasteur (1853), Stereochemistry (1874).
8. Raoult, Arrhenius (1886–7).
9. Radioactivity (Becquerel, Curies).

II. IDEAS CONCERNING AFFINITY.

1. Berthollet, Guldberg, Waage (1867).
2. Berzelius, Helmholtz (1887).
3. Mitscherlich, Spring (1904).
4. Deville, Debray, Berthelot.
5. Thomsen, Berthelot (1865).
6. Horstmann, Gibbs, Helmholtz.

I. PHYSICAL CHEMISTRY AND OUR IDEAS CONCERNING MATTER.

The Concepts of Atoms and Molecules. —Regarded as a whole, we may say that the initial application of physical knowledge for the purpose of developing our ideas of matter consisted chiefly in the employment of physical methods and instruments in the study of the properties of matter. This stood foremost in physical chemistry in the first period of its existence.

Reviewing the history of chemistry, we must acknowledge that one of the first fundamental steps was made by the study of the physical property of weight, and the introduction of a physical instrument, the balance, for this purpose. It was, in large part, on this basis that Lavoisier was the great innovator of chemistry; and it was due solely to the following of chemical change with the balance that chemistry got its fundamental laws of constant weight and of constant and multiple proportions. These were summarized by Dalton in the fruitful though hypothetical conception of atoms, which, as is well known to you all, asserts that every element exists in the form of small unchangeable particles, identical for a given element, but differing with the latter.

As the study of weight led to the idea of atoms, so the study of another physical property, that of volume and density, led to our idea of molecules. These molecules, which might be described as constellations of atoms, were a necessity with Dalton's conception; but, in a binary compound, for instance, they might consist of two atoms or of twenty. Now, it hardly needs to be recalled that Gay-Lussac, and especially Avogadro, in following the volume relations of gases in chemical action, drew the conclusion that the molecules of gases occupy equal volumes under identical conditions. Thenceforward we had a reliable method for determining the relative weights of such molecules.

As the study of the physical properties weight and volume led to the concepts of atoms and molecules, so sharply defined that the relative weights of these entities form the fundamental constants of chemistry, so a further study of physical properties has led to broad generalizations concerning the nature of atoms and molecules, which we shall now outline.

Properties of Atoms.—As to atoms, I would call your attention to four peculiarities which seem to me of fundamental importance. First, Dulong and Petit found that the physical property called heat capacity is nearly the same for different atoms, *i. e.*, that the quantity of heat requisite to produce a given rise of temperature does not vary greatly for atomic quantities, for 7 parts of lithium and for 240 parts of uranium.

Second, Faraday, in studying the electrical conductivity of electrolytes, *e. g.*, of aqueous solutions of salts, found that the quantity of electricity which atoms can transport varies as the whole numbers,—from one in potassium to two in zinc. This fundamental property, which gives the sharpest expression to our notion of valency, was brought by Helmholtz into a very clear form by the assumption that electricity as well as matter consists of

atoms, either negative or positive, and that material atoms are able to combine with them—potassium with one of the positive kind, zinc with two, chlorine with a negative one,—and so transport them in electrolysis.

The third great step was made by the study of light, a physical property again. Bunsen and Kirchhoff found that, heated in the gaseous state, every atom emits a definite set of light waves, producing a characteristic line-spectrum which is yet the sharpest test of the kind of atoms one is dealing with, and which so became the most fruitful guide in the detection of new kinds.

The last generalization that I have to mention, and which we owe to Newlands, Mendeléeff and Lothar Meyer, includes physical properties in general, and asserts that they vary with increasing atomic weight in a periodic way. This shows itself most sharply in the atomic volume, which passes through maximum values in lithium (7), sodium (23), potassium (39), rubidium (85) and cæsium (133). A corresponding periodicity is observed in other properties, as, for example, that of combining with electrical atoms or valency, which in the said elements passes through unity. Analogous behavior is exhibited by the melting points and boiling points, which for these metals are exceptionally low.

If my program did not to a certain extent exclude quite recent investigations, confining me to a view of past history, I should like to consider one more physical property, that of radioactivity, which also seems to be a property of atoms. I can only insist on the fact that it was physical properties again, the making the air conductive for electricity, and the spectrum, which revealed radium.

Properties of Molecules.—Turning to molecules, I have three predominant generalizations to outline. The first is Mitscherlich's discovery of the fact that analogous molecular constitution corresponds to analogous outer crystalline form, to so-called isomorphism. Let me add that there is hardly any more satisfactory proof of the soundness of our concept of the internal structure of matter than, *e. g.*, the identity of the crystalline forms of the alums, which we consider to have corresponding internal structure.

A second step, to a certain extent a similar one, was made by Pasteur when he deduced disymmetry of molecular constitution from disymmetry in behavior, optically as well as crystallographically. For instance, the dextrorotatory ordinary tartaric acid and its lævorotatory antipode showed this disymmetry both in optical rotation and in the particular so-called enantiomorphous crystalline form. The molecules were supposed to have analogous structures differing from each other as the right hand from the left. As is well known, it was only later that the probable molecular structure was sharply defined, and stereochemistry was founded.

The third great step was the opening of a way to determine the molecular weights of dissolved substances. It was chiefly the application of Avogadro's law to osmotic pressures, in connection with Raoult's measurements of freezing points and vapor pressures, that opened the way. We may now assert that the liquid state is not characterized by high molecular complexity. But the great innovation, introduced by Arrhenius and immediately brought into relation with the achievement in question, was the admission of the existence of ions in electrolytes—for example, the presence of negatively charged chlorine atoms and positively charged sodium atoms in an ordinary salt solution. Once more it was a physical property, the

electrical conductivity, that led to this extremely fruitful supposition.

Conclusion.—If, after this short summary of its properties, we try to look into the nature of matter, we conclude that matter is not continuous, but that there are centers of action which seem to have an eternal existence, changing only in the place that they occupy—these are the atoms. They keep together in some way and form the molecule; how, it is pretty hard to say. The planetary constellation, with ordinary attraction and centrifugal force in equilibrium, is excluded by the consideration that at the absolute zero there is no movement at all. The repulsive force that we want might be of electrical nature; and so we come to our combination of material and electrical atoms. There is indeed something fascinating here, and when we admit for carbon that it may unite to four equally charged electrical atoms and hold them by a force of the nature of elasticity, we have at once a possible equilibrium and the tetrahedral grouping. My only difficulty is that an uncharged atom of carbon, coming into contact with the ions just described, would take away half the electric charge, and so the valency of any element might be reduced to unity. The latest supposition, that matter is built up of electricity alone, lies again beyond the scope of this address.

Let me now turn to the second part of my subject, and touch upon the problem of affinity; indeed, the action that keeps atoms together must be closely related to affinity.

II. PHYSICAL CHEMISTRY AND OUR NOTIONS CONCERNING AFFINITY.

While physical chemistry, in the first period of its development, was chiefly devoted to the study of the physical properties of matter, the second and present period is characterized by the predominant place of the problem of affinity.

This change in the general aspect of our science goes hand in hand with a different way of working: in the development of our ideas of matter, physical chemistry introduced physical methods and instruments for the study of physical properties; in the development of our ideas of affinity, physical chemistry has introduced physical principles.

Affinity Considered as Force.—The first line of thought considered affinity as a force, and in this direction it was natural to think of the Newtonian attraction as the chemical agent. So it was that Berthollet, and with far more success Guldberg and Waage, applied the laws of mass action to problems of affinity, formulating a relation still known as the mass law, according to which affinity is proportional to the weight in the unit of volume.

Now, as we all know, affinity is of a specific nature, and does not depend on weight merely; on the contrary, the least heavy elements are generally the most active. So Berzelius built up his system founded on the notion that elements have a specific electrical character, either positive or negative, and, in combining, act by electrical attraction. In this direction Helmholtz made a further step in taking into account the quantitative side. Considering the electrical charges involved in Faraday's law, he pointed out as very important that the attraction due, for instance, to the negative charge in chlorine and the positive one in hydrogen far exceeds the gravitational attraction of the masses. Yet a satisfying notion of affinity was not obtained in this way.

Affinity Measured as Work.—A second line of thought took into consideration not the force but the work that affinity represents; and it seemed a decisive step when Thomsen and Berthelot declared that the

heat developed in chemical change corresponds to the work that affinity can produce. Indeed, it was in this way that in many cases an *à priori* calculation of the heat development of a reaction permitted prediction of the direction in which the process would proceed, the direction being that of the evolution of heat. Yet, this principle, however weighty, is not absolutely reliable. The chemical actions that produce cold, as that of hydrochloric acid on sodium sulphate, are objections not to be overcome.

The step really leading to a clear and unobjectionable notion of affinity was made in the study of the so-called reversible chemical changes. This reversible character perhaps needs some explanation, easily to be provided by an illustration. Kill a chicken and prepare chicken soup; it would then be very difficult to get your chicken again. This is because preparing chicken soup is not reversible. On the contrary, let water evaporate or freeze; it will be easy to reproduce the water.

Now, at first sight, chemical change does not seem reversible; and indeed it often is not, as in the explosion of gunpowder. But investigations of Berthelot and Péan de St. Gilles on the mutual action of acids and alcohols, and those of Deville and Debray on high temperature action, which even splits up water, have shown that many chemical changes can be reversed. Indeed, we have types corresponding absolutely to evaporation, as the loss of water vapor from hydrates; and others corresponding as well to freezing and melting, as the splitting of double salts into their components at definite temperatures, *e. g.*, copper calcium acetate at 77° C. Also in analogy with physical phenomena, we have in these reversible chemical changes the possibility of equilibrium, the two chemically different forms of matter coexisting, as do water and its vapor at a maximum pressure.

Such a reversal of chemical change can take place under the influence of temperature, of electricity, of light, of pressure. And the easiest way to arrive at a measure of affinity is presented in the last case, as was foreseen by Mitscherlich. Let us take gypsum as an example. Burnt commercial gypsum, mixed with water, will combine with the water. We know that this chemical change can produce pressure, and that it may be prevented by sufficient pressure and be reversed by it, as Spring succeeded in pressing out sulphuric acid from sodium bisulphate. And it is possible in such cases exactly to determine the limiting pressure, such that a higher one presses out the sulphuric acid while a lower one is overpowered by the affinity action. If the chemical change takes place under a pressure only slightly less than that which would prevent it, thus practically taking place under the limiting pressure, we get out of affinity the greatest quantity of work that it can possibly produce; and this quantity is the same whatever the nature of the opposing action, be it electricity, light, or anything else. Therefore, in this maximum work we have a sound measure of affinity.

It was a very happy coincidence indeed, that this conception of affinity made possible the application of a physical principle known as the second law of thermodynamics. This principle may be formulated in different ways. For my purpose let me say that it limits the possibility of natural processes to the occurrence of those in which a difference of intensity is diminished. If there is a difference of pressure in two parts of a gas, a movement will occur producing equality; if there is a difference of temperature, heat will be transported so as to produce equality once more. It is curious that such simple neces-

sities, which we all feel as such, can be converted into far-reaching sharply formulated equations, as was done by Carnot and Clausius. These principles were first applied in chemistry by Horstmann. Then, by successive application to chemical problems by Massieu, Gibbs, Helmholtz and others, was won a system of relations touching the problem of affinity, to which I can give only brief attention:

1. Affinity may be defined as the maximum quantity of work that a chemical change can produce. Equilibrium ensues when this quantity is zero.

2. The mass law can be obtained in a well-founded and somewhat modified form, restricted to dilute gases and solutions.

3. The Thomsen-Berthelot principle assumes a modified form in the rule that a fall of temperature induces the formation of that which develops heat. It is, for instance, in accordance with this rule that at ordinary temperatures water is stable in comparison with detonating gas, and that at high temperatures this relation is reversed, as it was found by Deville to be.

4. Lastly, we have the phase rule, indicating, for example, in what cases chemical phenomena will be comparable with melting and freezing, and in what cases they will be comparable with evaporation and condensation.

Most curious of all, we can treat problems of affinity in an absolutely trustworthy way, so that our calculations furnish a check upon experiment, without admitting anything concerning the nature of affinity or of the matter wherein the affinity is supposed to reside.

J. H. van 't Hoff.

THE PROBLEM OF RENAL FUNCTION.[1]

In my first lecture I touched upon a series of physiological problems that have been elucidated in a pharmacological way. I treated these problems in a merely cursory manner and did not enter upon details of the various investigations. Permit me to-day to discuss more fully a problem which has for a long time claimed my attention and which has for many years been a topic of research in my laboratory, namely, the problem of renal function.

As is well known, there are two leading and opposing theories on the nature of urinary secretion. According to one of these theories, which was developed most fully by Heidenhain, we have to deal with a true secretory process by which water and perhaps the salts pass through the glomerulus, whereas the specific constituents of the urine are liberated from the tubules so that the sum of both secretions is represented by the outflowing urine. According to the other hypothesis, which was first proposed by Ludwig and subsequently modified (in a biological sense) by his successors, there goes on in the kidney, side by side with the glomerular activity, dependent essentially on the mechanical conditions of the circulation, and independently also on the secretion of certain urinary constituents, a process of resorption in the urinary tubules. Through this resorption the slightly concentrated secretion of the glomerulus, corresponding to the water of the blood, undergoes concentration to a point characteristic of the urine.

The output of urine is chiefly conditioned on the largely physical excretory process, which, on account of its dependence on the blood flow, and the blood pressure in the kidneys, one is justified in regarding as a kind of filtration or transudation. On the other hand, the resorption of water through the tubules is not directly dependent on the circulation of the blood. That is, it is in nowise proportional to the abundance of

[1] Being the second of the Herter Lectures delivered at the Johns Hopkins Medical School.

the glomerular filtration. It would be more nearly correct to say that this process is inversely proportional to the filtration. There is, therefore, the more abundant and unconcentrated urine when the blood flow is more abundant, and, on the contrary, a more scanty and concentrated urine when the blood flow is scanty.

The Ludwig theory, as you are aware, is based chiefly on the directly evident dependence of the urinary secretion on the blood stream through the kidneys and on the blood pressure, and in fact this connection is a striking one. I will remind you of the experiments of Goll, which were conducted as long ago as the year 1854, in which the tension in the vessels was lowered by vagus irritation or by bleeding, or elevated by clamping the large vessels of the extremities, in which the volume of the urine rose and fell according to the increased or diminished flow of blood through the kidneys.

Against the whole theory of Ludwig, supported as it was on many other facts and arguments, Heidenhain brought a series of objections, of which the following was especially impressive. If the human blood holds about one tenth of one per cent. of urea and if it be estimated that the daily excretion averages about 30 grams of urea, it would follow that on the smallest estimate 30 liters of fluid must filter through the kidneys in twenty-four hours, for which about 28 liters of water must be reabsorbed. But if, according to the calculation of Heidenhain, not more than 130 litres of blood pass through the kidneys in the course of a day, it follows that according to Ludwig's view about one quarter of this volume filters out—a condition which would lead to a wholly impossible concentration of the blood in the glomerulus. In addition to this objection was urged the uneconomical work involved in the resorption through the urinary tubules of such great quantities of superfluous water. But we now know through the careful investigations of Tigerstedt that there flows through the kidneys during a very moderate diuresis in one minute a quantity of blood corresponding to 80–100 per cent. of their weight, or, in other words, since the human kidneys weigh about 300 grams, these organs are traversed by about 240–300 grams of blood in a minute, equivalent to 345–430 liters in twenty-four hours. Thus it is only about one twelfth to one fourteenth of the volume of blood and not one fourth of its volume, that is expressed. And so far as the chemical work of resorption is concerned, it must be remembered that the work of excreting 30 grams of urea through the elective action of the tubular epithelium from blood holding only 0.1 per cent. of urea must be exactly as great as the corresponding work of resorption by the same cells—in order to concentrate a 0.1 per cent. blood filtrate to 1.5 per cent. of urea through the resorption of water. Now Heidenhain endeavored to demonstrate the specific secretory function of the epithelia by means of injection of coloring matters. He injected into the veins of a rabbit a definite quantity of sodium sulphindigotate and removed the kidneys after a certain period of time, injected the vessels with alcohol and examined the structures histologically. He then found under certain conditions that the glomeruli were wholly free from coloring matter; the epithelia of the tubules, on the other hand, were colored. And he concluded from this that the dye was not secreted from the glomerulus but was secreted by the tubules. And from these observations Heidenhain made the inference that the epithelia of the tubules secrete also other constituents of the urine. These experiments are so well known that

I do not need to enter more fully upon them; what concerns us here is that they secured a wide acceptance for the Heidenhain theory. But since the pharmacological study of glandular secretion as compared with renal activity has shown that these forms of cellular function are different and even opposed to one another in certain respects, and since the behavior of certain pharmacological agents is difficult to harmonize with the Heidenhain view and easier to bring into accord with the theory of Ludwig, a number of pharmacological studies have been undertaken with a view to testing the validity of the Ludwig theory. If, as Heidenhain maintained, the coloration of the kidney is an indication and measure of its normal secretory activity, it might be expected that by means of experimentally increased diuresis this coloration would show corresponding alterations, that is to say, coloration would be intensified in the tubules. With this idea in mind Sobieranski, about ten years ago, began a new study of this subject in my laboratory by means of color injections, carried out according to the Heidenhain method. His results on normal animals led him to the conclusion that dyes were not excreted by the convoluted tubules, as Heidenhain thought from his findings, but in company with the water stream through the glomeruli, whence the dye passed into the tubules and was absorbed into the epithelium, coloring their nuclei. Through the simultaneous reabsorption of water by the tubules, the dye-stuff solution becomes more and more concentrated so that it (the dye) under certain conditions is bound to be separated in a crystalline state in the epithelium.

This had already been shown to be the case by experiments with sodium sulphindigotate, but still more clearly by means of experiments with carmine, a dye which is better adapted to this kind of experimentation because it does not undergo reduction (with loss of color) in the tissues. Even more surprising and striking were the results obtained on diuresis under the influence of caffeine, sodium nitrate or urea. Here the tubules were found to be very little or not at all colored. It was thus shown that an increase in the secretion of the dye through the tubular epithelium, which the hypothesis of Heidenhain calls for, under these circumstances did not exist. On the other hand, the process becomes intelligible through the explanation of Sobieranski. These diuretics give rise to diuresis by inhibiting the absorption of water from the urinary tubules, and at the same time prevent the absorption of the coloring matter by the tubular structures. Against this interpretation of Sobieranski's findings it is possible to offer certain objections, the validity of which we have recognized from the beginning. The experiments of Sobieranski can be regarded as corroborative evidence of the resorption function of the urinary tubules, but not as positive proof of this function.

One may approach the subject, also, from an entirely different side. If the process of separating water from the blood in the glomeruli is not an elective secretory process, as in other glands, but is in reality a process analogous to filtration, that is to say, is essentially dependent on physicochemical conditions, then one would expect, as Tammann has already shown, that together with the free water of the blood (not held by the blood colloids) the dissolved crystalloid constituents, like urea and salts, would simultaneously filter through; in other words, that with increased separation of water these bodies would also be excreted in increased amounts. On the other hand, the colloids and other substances similarly held in the

blood, which can not transude or filter through the normal glomeruli are not driven through with the water flow. They must rather be secreted through specific cell activity and quite independently of all mechanical filtration.

Indeed, we have known for a long time, from purely clinical observations, that some urinary constituents, like urea and sodium chloride, are excreted almost proportionately to the volume of the urine; and that others, on the contrary, like uric acid and phosphoric acid, are not influenced by the quantity of urine. In order to examine this problem in a quantitative way, a series of observations has been made by my assistant and collaborator, Otto Loewi. These experiments were made on dogs and rabbits in the following manner: The normal excretion of uric acid, urea and phosphoric acid was studied during a preliminary period of several hours. In still other cases the excretion of sugar was studied—indeed, not merely in diabetes following pancreas extirpation, but also after phlorhizin administration and intravenous injections of sugar. Then the secretion of urine was increased experimentally through free administration of water or by means of diuretics, such as sodium nitrate or caffeine, and during the diuresis so induced the above-mentioned constituents were quantitatively determined. Finally similar observations were made during and after the period following the cessation of the diuresis. The outcome was that the excretion of urea and of chlorides ran regularly parallel with the volume of the urine. On the other hand, there was never observed any parallelism between the excretion of water, on the one hand, and the increased amount of uric acid and phosphoric acid normally produced in the organism, on the other. I specially emphasize the phosphoric acid normally produced in the organism, for any phosphoric acid introduced as a salt into the circulation showed a different behavior. Such introduced salts followed the same law of excretion as the chloride and urea; and this same general law held true in the case of sugar.

The blood, as is well known, always contains sugar, but in a combined form, so that the sugar under normal conditions is not excreted by the kidneys. But after pancreas extirpation or after an intravenous infusion of sugar in normal animals, the sugar content of the blood rises above the normal; the greater part of it can not exist in combination in the blood but is free and, like urea and other crystalloids, is excreted by the urine. And it appears from Loewi's experiments on diuresis that in such pancreatic or infusion diabetes, the quantity of excreted sugar was always proportional to the volume of urine excreted. In phlorhizin diabetes, on the other hand, the behavior was entirely different. As you are aware, there occurs no hyperglycaemia in phlorhizin glycosuria. There is no increase of free sugar in the blood, but the normally combined sugar is liberated from its combination and excreted from the kidneys. And this specific cellular sugar excretion was shown to be quite independent of the filtration of fluid through the glomeruli, that is independent of the amount of diuresis. From this it seems to follow, in fact, that the substances which exist free in the blood pass out mechanically with the water; while other bodies, such as uric acid, phosphoric acid, phlorhizin-sugar, and probably the urinary pigments, are excreted from the kidney by special secretory activity. It is not necessary that a substance exist in a crystalline state in order that it be secreted by mechanical filtration through the glomeruli; it may equally well be a colloid, provided,

however, that it is not combined with the blood tissue. It has long been known that dissolved hemoglobin and injected albumin passed through the kidney into the urine. It is also shown by direct microscopical investigation that these bodies pass through the glomeruli. And, as in the case of urea and salts, this excretion of proteids through the glomeruli has been shown by experiments by Dr. Schmidt and Dr. Loewi in my laboratory to be a mechanical filtration.

This specific excretion of uric acid, etc., can not be increased by any of the known diuretics. The action of diuretics, therefore, can surely not be explained by the supposition that there is a stimulation of the secretory activity of the kidneys. And, moreover, a specific renal secretion is something quite different from the secretory activity of the glands, for the typical glandular poisons, like pilocarpine, not merely are without influence on diuresis in general, but have no effect whatever on the excretion of uric acid, phosphoric acid, etc. If, then, the filtration of the watery constituents of the blood is highly probable, it follows that as a means of saving water there must be a compensatory resorption in the tubules analogous to the process of resorption from the alimentary canal. In the case of the intestinal tract large quantities of fluid are secreted from the mouth, stomach and small intestines, even to the amount of several liters in twenty-four hours, which is later reabsorbed in the large intestine, especially in the colon, resulting in the semisolid fæces. And a similar process may be conceived to go on in the kidneys.

We have undertaken to determine whether this conception is correct. If under normal conditions such a process of concentration occurs in the tubules, one would expect a diarrhœa (that is, a flow of unconcentrated fluid from the blood) to result from elimination of or injury to these parts, just as there results a flow of watery fæces if the colon be removed or its cells paralyzed by poisons, or if the contents of the intestines are rendered incapable of absorption by the addition of certain salts. With this thought in mind Ribbert long ago conducted experiments involving the removal of the medulla of the kidney, as a result of which he did, in fact, observe an increased secretion of a very dilute urine in rabbits. Similar experiments were undertaken by Dr. Hausmann and myself three years ago, by means of a somewhat modified operative technique and especially with the aid of quantitative analysis of the urine. In rabbits from which the right kidney had been removed previous to operation on the left kidney, we found the excretion of urine increased three or four times, just as did Ribbert, and observed a change from a concentrated mucus-like urine before the operation to a light-colored watery urine of low specific gravity after the operation. Quantitative analysis further gave the noteworthy result that whatever might be the normal content of chlorides and urea, after the operation the content always approximated that of the blood serum; for example, if the normal content of sodium chloride in a diet poor in this salt averaged 0.1 per cent. or in a diet rich in sodium chloride approximated 2–3 per cent., after the operation there would result in each case a percentage of sodium chloride which varied within the narrow limits of 0.6 to 0.8 per cent. This result seemed to speak strongly for the resorption theory. The resorption activity of the kidney may be influenced by pharmacological means as well as by removal of the uriniferous tubules. One would expect, as in the case of the intestines, that salts would check resorption from the tubules, and, further, that as in the case of the intestines, the

bibasic would act more strongly than the monobasic salts. Comparative study of the action of Glauber salt and common salt injected into the blood vessels, made by Dr. Halsey, yielded the expected results. Results similar to these unpublished ones, have been obtained by Gottlieb and Magnus of Heidelberg, and especially by Cushny, of Ann Arbor. The isosmotic solution of Glauber salts possessed a much more strongly diuretic action than common salt. From these experiments we may conclude with Cushny that the salts prevent water resorption from the tubules and set up a kind of diarrhæa in proportion to their power of withdrawing water, and that hence in accordance with the Ludwig theory, we must assume that a resorption of water occurs under normal conditions. The matter is, however, not quite so simple, for the salts, owing to their ability to withdraw water, also withdraw water from the tissues into the blood, thereby increasing the filtration stream through the glomeruli. Cushny further showed that fixed constituents, like sodium chloride, may be reabsorbed, as in the case of the intestines, by the epithelia of the tubules, that the difficultly diffusible Glauber salt was only slightly and slowly reabsorbed, and that finally urea is apparently not reabsorbed at all. A further striking example of such a renal diarrhœa, as I am inclined to call it, has been brought forward by Loewi. As I have already stated, every diuresis dependent on increased glomerulus filtration occasions also an increased excretion of sodium chloride and urea. If one poisons an animal with phlorhizin there also occurs an increased diuresis. This phlorhizin diuresis, as Brodie showed, appears to be wholly independent, in general, of the circulation and especially of the circulation through the kidney. And, what particularly interests us here, this diuresis is not dependent on the filtration of water through the glomeruli, for, according to Loewi's analysis the chlorides and urea were not excreted in increased amount, as is the case in all the other forms of diuresis. The phlorhizin diuresis must be regarded, therefore, as a pure tubular diarrhœa, brought about by the sugar excreted in the tubules of the kidney itself and there hindering the resorption of water by means of its water-attracting properties.

We have, therefore, in many instances two closely connected processes which constitute the basis of increased diuresis, the interference with resorption from the tubules, and the increased filtration through the glomeruli, the latter being probably the more important factor. The question arises what are the conditions that determine the operation of these factors? It may be that to a slight extent the diminished viscosity of the blood or, more properly, the degree of saturation of the colloids of the blood with water are here concerned. We know that in thirsting animals the kidney secretion can not be increased in any way; we have, therefore, no quantitative conception of the extent of this influence. On the other hand, we know of one factor which is of determining significance for the process of filtration. This is the blood flow through the kidney. It was long ago shown by Roy that as a rule every diuresis is associated with an increase in the volume of the kidney, that is, sets in simultaneously with an increased blood flow, and the experiments of Roy have been repeatedly carried out with essentially the same results by numerous investigators, and especially by Gottlieb and Magnus and by Starling and Bayliss. Still it appeared from time to time that there were exceptions in which increased diuresis occurred in association with an unchanged volume or scarcely perceptible increase in the volume of the kid-

ney. Gottlieb and Magnus, therefore, felt justified in concluding that the increased blood flow through the kidney is not the primary and determining condition for increased diuresis, but rather a regular and not essential associated phenomenon. My collaborator, Professor Loewi, also carried on a large number of experiments in this direction. He, too, found that in certain cases the oncometer showed no increase in the volume of the kidney, notwithstanding an increase in diuresis. We further undertook to determine whether diuresis occurred under the influence of diuretic agents, like caffeine and salts, even when the volume of the kidney was fixed so that an increased blood flow is presumably prevented. For this purpose the left kidney of the rabbit in a relatively quiescent and relatively anemic condition was encased in plaster of Paris with the exception of the hilus only, so that an increase in the volume of the kidney was wholly excluded. The surprising result was obtained that even in the case of such rigidly enclosed kidneys, diuretics like caffeine and salts were able to induce an abundant diuresis. Hence it seemed to be actually true that an increased filtration may be induced without any increase in the blood flow through the kidneys. But more careful investigations showed that the volume of the kidney is by no means a certain measure of the blood flow through this organ, but that the volume of the kidney and the blood flow through it may be independent. For by inspection of the outflowing venous blood it could be seen that, notwithstanding the rigid limitation in the volume of the kidney, the flow of blood through the organ was always enhanced during diuresis. While the blood which flowed through the renal vein was dark previous to the diuresis, the stream took on a light, arterial color under the influence of caffeine and salts. The mere fact, therefore, that the kidney does not increase in size in some cases of caffeine diuresis is no proof that the process of diuresis does not depend on an increased flow of blood through the kidney, and one may say that an increased renal blood flow is a regular and essential condition of diuresis from salts, urea and caffeine—a condition wholly sufficient, in itself, to explain the diuresis. It is not possible to say with certainty whether in the case of caffeine diuresis there is also a diminished resorption of fluid through the urinary tubules, as Sobieranski's experiments appeared to show. Another important fact was brought out by Loewi in this connection; we know that every hydremia, whether induced by the administration of water or by the withdrawal of water from the tissues by means of salts intravenously injected, gives rise to an increased diuresis, without any increase in the general blood pressure or the work of the heart. What is the origin of such a diuresis? Loewi found that every hydremia, whatever may be its origin, acts upon the vessels of the kidney as a specific excitant, in that it dilates the vessels and thus causes an increased glomerular filtration. Thus we have obtained an explanation for the increased secretion of urine arising from all forms of hydremia, from the drinking of water, and from the withdrawal of water from the tissues, in consequence of the action of the diuretic salts. Hence we may say that all the observations that have come to us by physiological and pharmacological methods harmonize with the conception that the water of the blood and the free crystalloids therein dissolved are liberated from the glomeruli by the process of filtration or perhaps a process better described as transudation, and, further, that the urinary tubules reabsorb, by means of their epithelial cells, not only water, but also, in

cases of salt poverty, sodium chloride as well (these being materials which the organism can not afford to lose), while at the same time these epithelial cells, like those of the intestines, have also to perform the duty of excreting the combined substances of the blood by means of their specific secretory activity.

Diuresis, therefore, represents the fusion of two principal processes—one concerning the gomeruli, which is in its main features mechanical in its nature; the other pertaining to the urinary tubules, which is not yet explicable on any physico-chemical hypothesis. The process of resorption from the urinary tubules has a distinctly biological, that is, teleological character; water and salt are only reabsorbed when the organism does not possess these in excess. If one administers an abundance of water, the urine acquires a highly watery character, while after the administration of an abundance of sodium chloride there is a failure on the part of the tubules to reabsorb salt, as Loewi has shown. The process of reabsorption adapts itself, therefore, to the requirements of the organism.

Although I believe that the theory of renal function which I have here presented is the one which has the best experimental foundation, I readily concede that it leaves many facts still unexplained. For example, it is difficult by means of this hypothesis to explain the constitution of the urine in diabetes insipidus as well as the complete retention of chlorine under certain conditions, and I fancy that we shall have to suppose, as Cushny has done, that there is some kind of combination of sodium chloride with the blood tissue which hinders its filtration. The theory of diuresis and the action of diuretic drugs further possesses a practical interest. If, for example, it be true that caffeine acts diuretically through local specific dilation and not through irritation of the secretory cells, as was formerly supposed, then, as Loewi thinks, we are justified in its administration during long periods in the course of nephritis in which, in many instances, the vessels of the kidneys are abnormally contracted. And there is some reason to believe that the vasodilator action of the caffeine not merely induces an increased diuresis but exerts a favorable influence upon the pathological condition of the kidney itself.

In conclusion, I desire to express my appreciation of the courteous attention you have accorded me. HANS MEYER.

THE EVOLUTION OF SPECIES THROUGH CLIMATIC CONDITIONS.

IN a recent article in SCIENCE,[1] entitled 'The Origin of Species through Isolation,' President David Starr Jordan has presented much evidence bearing upon the influence of geographical isolation in the formation of species and races of animals and plants. He dwells especially upon the agency of barriers in interrupting the flow of life and isolating groups of individuals of a species, which groups of individuals, either with or without material change in the conditions of existence, 'may become in time an entirely distinct species if the barrier is really insurmountable.' This is impliedly recognized as only one of various influences that tend to modify species, but in this connection, in marshalling the evidence in favor of the proposition of the origin of species through isolation, hardly any reference is made to the part played by other agencies in the evolution of new forms. In this way, rather undue importance is given to a single and well-recognized factor in the problem of evolution. The purpose of the present paper is not to

[1] N. S., Vol. XXII., No. 566, November 3, 1905, pp. 545–562.

question or controvert any of the evidence, or in any way to belittle the principle of evolution by isolation, but to bring forward other well-known facts that bear on the problem of evolution through environment unassisted by isolation or obvious physical barriers.

Says President Jordan, in his opening paragraph:

> It is now nearly forty years since Moritz Wagner (1868) first made it clear that geographical isolation (räumliche Sonderung) was a factor or condition in the formation of every species, race or tribe of animal or plant we know on the face of the earth. This conclusion is accepted as almost self-evident by every competent student of species or of the geographical distribution of species. But to those who approach the subject of evolution from some other side the principles set forth by Wagner seem less clear. They have never been confuted, scarcely even attacked, so far as the present writer remembers, but in the literature of evolution of the present day they have been almost universally ignored. Nowadays much of our discussion turns on the question of whether or not minute favorable variations would enable their possessors little by little to gain on the parent stock, so that a new species would be established side by side with the old, or on whether a wide fluctuation or mutation would give rise to a new species which would hold its own in competition with its parent. In theory, either of these conditions might exist. In fact, both of them are virtually unknown. In nature a closely related distinct species is not often quite side by side with the old. It is simply next to it, geographically or geologically speaking, and the degree of distinction almost always bears a relation to the importance or the permanence of the barrier separating the supposed new stock from the parent stock.

With the above I am in hearty accord, except with the declaration that geographical isolation is a 'factor or condition in the formation of every species, race or tribe of animal or plant * * * on the face of the earth'; especially if the isolation here meant implies a physical barrier, 'geographical or climatic,' to the continuous range of closely allied forms, as the general context seems clearly to involve. That isolation is an important factor no intelligent biologist will be disposed to deny; that it is not the only important factor, or, as regards incipient species, the chief one involved there is ample evidence, well-known to a large number of present-day investigators.

President Jordan, however, appears to have mainly in mind species, or fully segregated forms, rather than incipient species, or intergrading geographic phases; as when he states, as 'practically a universal rule': "A barrier which prevents the intermingling of members of a species will with time alter the relative characters of the groups of individuals thus separated. These groups of individuals are incipient species and each may become in time an entirely distinct species if the barrier is really insurmountable."

In illustration of his theme, the author cites examples of mammals, birds, fishes and mollusks, which serve very well to illustrate the points at issue; except that incipient species, as well as fully segregated forms, are claimed to owe their existence to barriers, either geographic or climatic, which is not generally the case, taking the term 'barrier' in its commonly accepted sense.

Doubtless President Jordan well knows that among birds and mammals, and especially among the former, many wholly distinct congeneric species, or forms that are known not to intergrade, are often much more nearly alike, structurally and in color and size, than are the extremes in many groups of forms that are known to intergrade completely through geographically intervening forms, as notably in the song sparrow (*Melospiza cinerea*) group; and that many other conspecific groups of wide distribution show nearly parallel variations, as the horned larks, and, to a lesser

extent, many species of woodpeckers, jays, the bob-whites, some of the pipilos, juncos, wrens, titmice, etc. As to exploit all of these cases would require a good-sized volume, only two or three can be taken for illustration in the present connection.

In the first place, to facilitate treatment, certain general well-known laws of climatic or geographic variation may be recalled, which are of so nearly universal application that the exceptions, generally easily explainable, may be ignored. First, in relation to size. In the northern hemisphere, in nearly all types of both birds and mammals of obviously northern origin, there is a gradual decrease in general size from the north southward in the representatives of a conspecific group, most marked, in the case of birds, in non-migratory, or only partly migratory, species, the most southern representatives of such groups being from one fifth to one third or more smaller than the most northern representatives of the same groups. At the same time, but less generally, there is a relative increase in certain peripheral parts, as the length of the tail, the thickness or length of the bill (according to its form) in birds, and often the ear, tail and feet in mammals.[2]

Secondly, and coincidently with the decrease in size southward, is a change in coloration, which may be described in general terms as a restriction in area of all white markings and a corresponding increase in the area of the dark markings, together with, generally speaking, an increase in the intensity of color in markings or areas of other tints than black or white, as yellows, greens, browns, etc., and also in iridescence, in birds of metallic tints. The birds of latitudinally extended breeding ranges in eastern North America rarely present exceptions to these rules; and many

[2] As already intimated, there are some exceptions to the rule of decrease in size southward, which are covered by the following propositions, first formally propounded in 1876 (*Bull. Geol. and Geogr. Surv. Territories*, Vol. II., No. 4, July, 1876, p. 310) in relation primarily to mammals and later (*Bull. Nutt. Orn. Club*, Vol. III., April, 1883, pp. 80–82) restated with more direct reference to birds, as follows:

"(1) *The maximum physical development of the individual is attained where the conditions of environment are most favorable to the life of the species.* Species being primarily limited in their distribution by climatic conditions, their representatives living at or near either of their respective latitudinal boundaries are more or less unfavorably affected by the influences that finally limit the range of the species. * * * Different species being constitutionally fitted for different climatic conditions, surroundings favorable to one may be very unfavorable to others, even of the same family or genus. Hence:

"(2) *The largest species of a group* (genus, subfamily, or family, as the case may be) *are found where the group to which they severally belong reaches its highest development, or where it has what may be termed its center of distribution.* In other words, species of a given group attain their maximum size where the conditions of existence for the group in question are the most favorable, just as the largest representatives of a species are found where the conditions are most favorable for the existence of the species.

"(3) *The most 'typical' or most generalized representatives of a group are found also near its center of distribution, out-lying forms being generally more or less 'aberrant' or specialized.* Thus the Cervidæ, though nearly cosmopolitan in their distribution, attain their greatest development, both as respects the *size* and the *number* of the species, in the temperate portions of the northern hemisphere. The tropical species of this group are the smallest of its representatives. Those of the temperate and cold-temperate regions are the largest, where, too, the species are the most numerous. Most of the species of this family also have a wide geographical range, and their representatives respectively present great differences in size with locality, namely, a very marked decrease in size to the southward. The possession of large, branching, deciduous antlers forms one of the marked features of the family. These appendages attain their greatest development in the northern species, the tropical forms having them reduced almost to mere spikes, which in some species never pass beyond a rudimentary state. * * *"

of the mammals of the same region exhibit perfectly parallel variations. Nor are these rules restricted to eastern North America, but prevail throughout the northern hemisphere, and also in the southern hemisphere, with, however, the conditions reversed, the increase in size and intensity of colors being from the south northward. High mountains in low latitudes represent, in respect to these phenomena, the higher latitudes nearer sea-level.

It is equally well known that, in continentally dispersed groups, pallid tints accompany desert areas and arid conditions of climate, and that increase in depth of color, particularly in gray, brown and olive tints, is an inseparable accompaniment of regions of heavy rainfall and a moist climate, so familiarly illustrated in the northwest coast region of North America. Furthermore, there are various other areas in the world where the animal inhabitants are collectively characterized by some special feature of coloration, as excessive lightness of color—hoariness, or increase in area of light or white markings in eastern Siberia, or redness or blackness of coloration in parts of Africa, etc. In other words, regional areas of peculiar climatic conditions impress upon their animal inhabitants a certain distinctive phase of coloration, developing in some instances wholly new specific types, in others merely forms that intergrade with others of the immediately adjoining districts.

To return now to our starting point in eastern North America, the variation in size from the north southward is as gradual and continuous as the transition in climatic conditions; there are no barriers, in the ordinary sense, and no abrupt transitions. In conspecific groups the phase inhabiting Canada or the northern border of the United States, in species of wide breeding range, differs more from the phase inhabiting southern Florida and the Gulf coast than do many congeneric species known to be completely distinct; and were these phases isolated by a wide geographic interval they would have to be recognized in nomenclature as distinct species. It would also be the same if they were found living together, the differences between them are so marked. In reality, however, these very distinct phases are merely the extremes of a single continuous intergrading series or unbroken sequence of individuals of one and the same species.

In passing westward from the Atlantic seaboard to and across the arid interior of the continent, and thence to the Pacific coast and northward to Alaska, other forms of the same conspecific groups may come into view, so distinct from their eastern representatives and from each other, that many of them were originally described as distinct species, and for many years were so recognized, till the accumulation of material from many intermediate points showed them to be connected by insensible gradations over the intermediate regions, and that their true status was that of incipient species, or merely geographic forms, distinct enough when birds of distant and limited areas are compared, but inseparable when those of intermediate regions are taken into account. There are also, in these cases, no barriers beyond the gradual climatic transition from one area to another. This, at some points, owing to topographic conditions, may be abrupt, but in general is too diffused to prevent the continuous spread of the species.

In illustration of these points, we may take the downy and hairy woodpeckers (*Dryobates pubescens* and *D. villosus*), both species of continental distribution, and each with its half dozen or more commonly recognized subspecies, varying enormously in color and in size when those of

eastern North America are compared with each other, or either of these with the Rocky Mountain forms, and these again with the northwest coast and southern California birds.

The eastern flickers (*Colaptes auratus*), the western flickers (*C. cafer*), and the eastern pileated woodpecker (*Ceoplœus pileatus*), have also been divided into subspecies, mainly on the difference in size between the northern and southern representatives of the species. The bob-white (*Colinus virginianus*) has in the east its northern and southern forms, differing markedly in size and coloration, with another pallid form in Texas, which passes into a group of very diverse forms in different parts of Mexico, some of them having black instead of white throats, uniform chestnut instead of barred black and white underparts, and others with various other combinations of characters, yet all retaining their characteristic *bob-white* call, and so blending with each other and the northern forms as to be found to be specifically inseparable, though they have usually been accorded the status of species.

The horned larks (*Otocoris alpestris* group), ranging from the Arctic regions to northern South America, and embracing nearly a score of forms, some of them extremely diverse, are found to completely intergrade, though the various forms have special breeding areas, and have obviously attained their differentiations under the most diverse climatic conditions. Yet they are separated by no appreciable barriers, and contiguous forms completely intergrade, forming an unbroken chain from one extreme of the series to the other.

The almost hackneyed case (at least to ornithologists) of the song sparrows (*Melospiza cinerea* group), nearly continental in dispersal and differentiated into about twenty recognized forms, may well close this series of illustrations. Many of the forms of this group are so different that, when compared without the connecting links, they seem like remotely related species, almost different enough for subgeneric separation. Between the great gray song sparrow of the Aleutian Islands and the little song sparrow (*pusillula*) of the San Francisco salt marshes the difference in size is enormous, the length of the wing being 85 mm. in the Alaska bird and 56 mm. in the San Francisco bird, the northern bird having probably more than twice the bulk of the southern one. Between them are some eight Pacific coast forms, by means of which there is continuous gradation between these extremes. Between the desert forms of the interior and the coast form at Sitka, there is much less difference in size, but the former is a pale gray bird, while the latter is deep intense rufous.

The recognized Pacific coast forms, from Lower California to the Aleutian Islands, excluding several insular phases, are nine in number, between which there is no abrupt barrier, either climatic or geographic, and consequently we find the successive and gradual mergence of all; but if a few of the links were dropped out, the remaining forms might readily be taken for distinct, fully segregated species, so much do they mutually differ.

Almost numberless similar illustrations might be cited among North American mammals. In general, the geographic ranges of conspecific groups in mammals are less extended, but they are of sufficiently wide range to include a great variety of environmental conditions, which result in marked modifications of size, color and structure.

Indeed, mammals being sedentary, they are even more susceptible to climatic modification than birds. Some of the more

widely dispersed species of squirrels, spermophiles, voles, field rats and mice, hares, gophers, pouched rats and mice, deer, shrews, moles and bats, afford admirable illustrations of differentiation without isolation. To cite a single notable case, the Virginia deer ranges over eastern North America from New England and southern Canada to Central America, varying greatly but gradually, in size and other features, till in the southern forms the size is only about one half of that of the northern, with disproportionately reduced antlers.

Variations of this nature, however striking, can not be due to isolation, for in the cases here in view there is no isolation, but continuous distribution, and consequently complete intergradation. The amount of differentiation, between the more extreme phases in various directions, is great enough, were these several forms isolated, or separated by geographic intervals, to warrant their recognition as well-marked species. All that is requisite to constitute them species is the extinction, through some physical cataclysm or other cause, of the connecting links, over portions of the intermediate areas by which even the extremest phases are at present connected. Doubtless in the past many species, and perhaps genera, have had their origin in the dismemberment of such groups of incipient species, through violent physical changes in the distribution area of widely dispersed specific types.

At present these connecting links between the leading phases of highly diversified conspecific groups are a nuisance and a stumbling block to the systematist in labeling collections, since between each properly namable form there is an area, more or less extended, where none of the forms are typically represented, but which is occupied by troublesome intergrades, approaching one form, usually, more than another, and in various degrees. These intergrades, furthermore, offer a temptation to aspiring young naturalists to give them a name, thus 'bridging the difficulty by doubling it.' If these areas of intergradation could be transformed into areas of isolation many problems in nomenclature and much trouble in identifying specimens would be eliminated.

The facts already set forth explain why two or more subspecies are never found in the same breeding area—a proposition discussed at some length in the paper here under consideration. They also show why insular forms, if separated somewhat remotely from the parent stock, assume the character of species rather than subspecies, and owe their existence to isolation. But insular forms exhibit various degrees of distinctness from the parent stock, depending upon the completeness and duration of the period of isolation. Theoretically there can be no intergradation between insular forms, particularly in land mammals—not so clearly in the case of birds, with their exceptional powers of locomotion; but in the case of forms inhabiting islands but little removed from a neighboring mainland, the environing conditions may be so similar that not time enough has elapsed to develop a well-marked type through the action of isolation, in which case the normal range of individual variation in the insular and the stock forms may be sufficient to cause an overlapping of the characters through occasional individual aberration. This might be mistaken for actual intergradation, while it is in fact merely an overlapping through individual variation. To this condition is probably due the recent tendency to give slightly differentiated insular forms the rank of subspecies rather than that of full species, to which theoretically they should be entitled.

I have long been a believer, in common

with many of my fellow-systematists, in the evolution of species and races by environment, which, of course, includes evolution by isolation. This seems far more rational than evolution by natural selection, as this process was originally defined. But since the inefficiency of natural selection, pure and simple, to produce the results formerly attributed to it became evident the significance of the term has been expanded to embrace evolution through climatic influences, including also the factor of isolation. The inefficiency of natural selection, as originally defined, in the evolution of species and races was the subject of frequent comment by the present writer in various papers published some thirty years ago, in opposition to which was urged the direct action of environment in the origination of incipient species. The discussion was eventually summarized in a paper published in 1877, entitled 'The Influence of Physical Conditions in the Genesis of Species.' This paper, of some thirty-three pages, appeared in a short-lived and not widely known magazine,[3] and thus doubtless escaped the attention of many investigators interested in such problems. The facts of geographic variation were here presented at some length.

In discussing Darwin's statement[4] regarding the method of the action of natural selection, namely: 'I believe that natural selection generally acts slowly in effecting changes, *at long intervals of time, and only on a few of the inhabitants of the same region*,' the direct and simultaneous action of climatic conditions upon all of the individuals inhabiting the same region was dwelt upon at length. Thus, to quote a few passages:

The local races of any given region, as compared collectively with those of contiguous regions, and the manner of their mutual intergradation, point plainly to some general or widely acting cause of differentiation. This is indicated by the constancy of the results, so many species, belonging to numerous and widely distinct groups, being similarly affected. * * * There is, however, a vast amount of unquestionable proof of the direct and constant action of climate and other conditions of life upon animals, and that such geographical variations as the thicker and softer fur of mammals inhabiting cold regions, smaller size and brighter colors at the southward, etc., do not require the action of natural selection, in its strict and proper sense, for their explanation. It is well known, for instance, that a flock of fine-wooled sheep, when taken to a hot climate, rapidly acquire a coarser and coarser fleece, till, in a few generations, it nearly loses its character of proper wool, and becomes simply hair; that the change affects simultaneously the whole flock, and is not brought about by one or two individuals acquiring a coarser fleece and through their descendants modifying the character of the herd. Furthermore, in the case of sheep, it is well known that certain countries are very favorable to the production of a fine fleece, and that fine-wooled breeds, even by man's aid, can not be perpetuated in other regions. Again, it is a fact of common observation that in birds and mammals colors become more or less faded toward the moulting season simply by the direct action of the elements—the tints of the fresh and the long-worn plumage or pelage being more or less strikingly different in the same individuals—and that this contrast at different seasons is more marked in arid than in moist regions, through the greater bleaching effect of a dry heated atmosphere and the more intense dazzling sunlight of regions that are not only cloudless, but lack the protection afforded by abundant vegetation.

But climatic conditions were not claimed in this essay as the only agent in the evolution of species and races. President Jordan has referred to the song birds of the family Drepanidæ in the Hawaiian Islands, with such remarkable diversity in the form of the bill and other features, which he ascribes, perhaps properly enough, in part to natural selection and in part to isolation. Upon such cases the following paragraph from my long-forgotten paper has some bearing:

[3] *The Radical Review*, No. 1, May, 1877, pp. 108–140.

[4] 'Origin of Species,' 5th ed., p. 168.

While so much is claimed * * * as due to climatic causes, it is admitted also that habits and food, and other conditions of life than those resulting from climate, have a marked effect in determining modifications of form and color among animals. A scarcity of a favorite kind of food will undoubtedly force species to subsist upon the next best that offers, which may be so different as to modify certain characters and fit the species to live upon the less desired food. A change of food may lead to modification of dentition, the muscles of mastication, and the organs of digestion, and, correlatively of other organs or parts of the body; the modification, however, arising simultaneously among all the descendants of the individuals thus driven to a change of diet, instead of appearing first in a single individual and becoming perpetuated in its descendants alone. Entomologists have found that, among insects of the same species, the forced or voluntary use of different food-plants gives rise to modifications of color and structure, and hence results in what have been termed phytophagic varieties or subspecies, and that man can also affect such changes at will by simply changing the food of the species. Again, the geological character of a country is well known to have a marked effect upon the size and color of animals inhabiting it, as is strikingly illustrated among molluscous animals, whose abundance, and even presence, is largely dependent upon the constituents of the soil. Over regions of the United States, for example, where the underlying rock is non-calcareous, the species are both few in number and sparsely represented. In respect to the fresh-water mussels, those of the same species from different streams are easily distinguishable by differences in the thickness of the shell, in color, shape, and ornamentation, so that the character of the shells themselves affords a clew to the locality of their origin. At some localities the species tend to become tuberculous or spinous * * *; at other localities, they acquire a very much thickened shell, or different colors, the same characteristics appearing simultaneously in quite diverse species, and thus becoming distinctive of particular localities. [After reference to mammals of certain regions being influenced in relation to size by the presence of calcareous or non-calcareous soils, and to the birds of the Galapagos Archipelago, with their short wings and large bills, etc., there follows:] The sedentary life necessitated by the confined habitats of species thus situated would naturally act more or less strongly on the organs of flight, and a reduction in the size of the wing would follow; not necessarily through the round-about process of natural selection, through the modification originally of a single individual, but by the direct action on all the individuals alike of the changed conditions of life.

There are thus what may be termed regional modifications due to the direct action of environment, independently of natural selection, in its original, restricted sense, or of isolation. The modifying influence may be either primarily climatic or due to peculiar constituents of the water or soil and the resultant vegetation. In a sense the two latter conditions may act as barriers, with the resultant effect of modified isolation. In general, however, in birds and mammals, in which regional modifications are so patent, the main factor is climate, the action general, and the transition between regions gradual. While all these influences may be as active on islands as on continents, there is in the former the powerful agency of isolation superimposed upon all the other agents that tend to the differentiation of animal forms.

J. A. Allen.

SCIENTIFIC BOOKS.

The Evolution Theory. By Dr. August Weismann, Professor of Zoology in the University of Freiberg in Breisgau. Translated with the author's cooperation by J. Arthur Thomson, Regius Professor of Natural History in the University of Aberdeen, and Margaret R. Thomson. London, Edward Arnold. 1904. 2 vols., illustrated. Pp. 416 and 405.

No one, in the last thirty years, has contributed more to the discussion and investigation of evolutionary problems than has August Weismann. The present work marks the culmination of his long series of stimulating writings. His fertility in hypotheses and keenness of criticism are well known; not less noteworthy is his readiness to withdraw hypotheses when disproved, or to modify them to conform with new discoveries. Thus, the

reader of 'The Evolution Theory' will note many minor changes from the positions taken earlier by Weismann in 'The Germ Plasm,' yet the substance of his theory of evolution remains unchanged.

The fundamental idea in 'The Germ Plasm' was the mutual independence of soma and germ, that is, of the body exclusive of the reproductive cells on one hand, and the reproductive cells on the other hand. Each, it was maintained, might be modified without modification of the other. This idea, at the time a novel one, has been shown by subsequent investigations to be substantially correct. It is the great merit of Weismann to have inspired those investigations. Through experimental studies, in which American zoologists have borne an honorable part, the effects of various external agencies upon the soma have been carefully analyzed. What effect, if any, these external agencies have upon the germ plasm is less clear. The opponents of Weismann, in common with Darwin, have at times maintained that induced modifications of the soma were handed on directly to the germ plasm and thus became hereditary. Weismann has always denied any such modification of the germ plasm through the soma, but concedes a modification of the germ plasm *parallel* with that which is directly induced by the environment in the soma. The germ plasm, however, in his opinion, is less sensitive than the soma to environmental changes, and so responds only to continuous influences, not to those which last for a single generation only. In this way Weismann seeks to find a basis for the innumerable and often marvelously perfect adaptations of organisms to their environment.

Weismann insists upon the germinal origin of variations which are heritable, but concedes that germinal variation may be given a particular direction by the environment. These variations may at first be too slight to have selectional value, but by the persistent action of the environment will be increased until selectional value is attained. Further, they will make their appearance not in an occasional individual merely, as we should expect if they are due to chance, but in so much of the race as is subjected to the continuous influence of the same environment. In taking this position Weismann attaches less importance than formerly to natural selection, adopts a different conception of the origin of variations which are heritable, and accounts more fully for adaptations.

Weismann shows his open-mindedness and breadth of view in adopting from his opponents an idea upon which paleontologists have laid stress, that when an organism has once begun to vary in a given direction, there is an inherent internal tendency for it to go on varying in that same direction, this tendency being quite independent of the environment and due to a struggle of the determinants of the germ plasm among themselves, a process which Weismann calls germinal selection. To this position he has been led by two considerations: (1) that there occur among organisms adaptations too subtle and complicated to have selectional value in the struggle for existence, and (2) that many organisms are over-adapted, that is, have progressed beyond what is advantageous in a particular sort of adaptive variation, as, for example, the extinct Irish elk with his tree-like antlers.

'The Evolution Theory,' while containing a full exposition of Weismann's own views, includes much else. It contains an accurate and interesting historical account of the development of the evolutionary idea from its origin in the speculations of Greek philosophers to its culmination in Darwin's 'Origin of Species.' A very full account is given of Darwin's views and of the lines of evidence on which those views were based. The mutation theory of de Vries is critically examined, though it finds little favor in Weismann's eyes. Like Darwin, he considers sport variation (mutation) of small consequence in the production of species, believing its effects to be local and temporary, resulting in the production of small and peculiar groups within species, but not of the broader species groups themselves. In breadth of scope and fullness of treatment Weismann's book surpasses all other works on the same subject; it will doubtless long remain the authoritative statement of Darwinism.

The translation from the second German edition has been executed with rare skill and fidelity. The work of the publisher is also good.

W. E. Castle.

Bacteria in Relation to Plant Diseases. By Erwin F. Smith, in charge of Laboratory of Plant Pathology, Office of Physiology and Pathology, Bureau of Plant Industry, U. S. Department of Agriculture. Volume I., Methods of Work, and general literature of Bacteriology exclusive of Plant Diseases. Washington, D. C., published by the Carnegie Institution of Washington. September, 1905. Pp. xii + 285. 4to. Publication No. 27.

We are told in the preface that "the present volume contains an 'outline of methods of work' which was written up in substantially the same form four years ago, in connection with the investigations of the Laboratory of Plant Pathology, Bureau of Plant Industry, United States Department of Agriculture, its publication having been delayed in order to bring the rest of the manuscript into suitable shape." In its present form it is now published 'with the approval of the Secretary of Agriculture.' This book has thus a quasi-official authority, representing, as it does, the high standards set by the scientific bureaus of Washington.

The author says that his monograph 'is not intended to take the place of ordinary text-books of bacteriology, but rather to supplement them.' While primarily intended for the plant pathologist, 'it is hoped that it will be of value to physicians and animal pathologists for purposes of comparison.'

The principal topics touched upon in this volume are the nature of disease, the morphology, physiology and pathogenic character of the organism, the preparation and use of various kinds of culture media, economic aspects, methods of infection, methods of prevention, location and equipment of the laboratory, methods of work, microscopes, nomenclature and classification, working formulae etc. At the close of the book there is a classified bibliography including almost fourteen hundred titles, which must prove of the greatest value to the bacteriologist.

Turning to the section which deals with nomenclature and classification, one reads with a smile the crisp remarks of the author, as when he says 'the nomenclature of the bacteria is in a somewhat chaotic state, as might be expected of a science which has been cultivated so largely by medical men, and so comparatively little by systematic botanists and zoologists.' The designation of species by numbers and letters is condemned, as also the use of polynomials. Better descriptions are strongly urged, and far more care in associating a particular organism with a certain disease. The suggestion is made that the starting point for species should be 1881, when pure cultures became possible. The suggestion is made, also, that the starting point for genera should be 1872, the date of Cohn's 'Untersuchungen über Bakterien.' With some modifications the author adopts Migula's plan of classification in his 'System der Bakterien,' 'until some distinctly better system makes its appearance.'

On the question of the polymorphism or fixity of bacteria Dr. Smith holds 'a sort of middle ground':

> There can be no doubt that the same organism sometimes exists as a long filament in which no septa are visible, and at other times as a short or nearly isodiametric rod, but we are not thereby compelled to consider the short form as a *Micrococcus*, *i. e.*, as something very different from the long form. Physical conditions probably have much to do with bringing about these differences.

We should like to quote further from this very suggestive and helpful book, but must refer the reader to the volume itself. The value of this volume is so evident that we look with great interest for the second, whose publication it is hoped will not be long delayed.

Charles E. Bessey.

The University of Nebraska.

SCIENTIFIC JOURNALS AND ARTICLES.

The *Journal of Nervous and Mental Diseases* for November opens with a paper by D'Orsay Hecht in which he reviews the literature of dementia præcox, frequently illustrating the

points mentioned with cases from his own experience. The article is to be continued in the next issue. Dr. Ward A. Holden presents some statistics on the early ocular signs of dementia paralytica, and sums up as follows: "In true, uncomplicated paresis there is early in the disease almost constant absence of the sensory reflex, in half the cases irregularity of the pupils, in nearly half inequality of the pupils, in more than half abnormally small pupils, in a fifth of the cases loss of light reaction, in another fifth marked sluggishness of light reaction, and in a few of those with diminished light reaction a diminution of convergence reaction also. A preliminary paper on psychasthenia, its clinical entity illustrated by a case, is contributed by Sidney Schwab."

BEGINNING in January next, it is announced that the *Bulletin of the Geographical Society of Philadelphia* will be issued every three months. Professor Emory R. Johnson will be the responsible editor and will have the cooperation of Mr. Walter Sheldon Tower as associate editor. The past year has been the most prosperous one in the history of the society—judging by the increase in membership and general activity of the organization.

SOCIETIES AND ACADEMIES.

THE CENTRAL ASSOCIATION OF SCIENCE AND MATHEMATICS TEACHERS.

THE association will hold its annual meeting on December 1 and 2 in the Y. M. C. A. building, 153 La Salle St., Chicago. The general meetings will be held on the second floor, and the section meetings on the seventh floor, of the building. The association, which has members in twelve states, invites all persons who are interested in the subject matter and the teaching of science and mathematics to become members. The program which follows may be had in pamphlet form upon application to the secretary, C. M. Turton, 440 Kenwood Terrace, Chicago, Ill.

GENERAL PROGRAM.

Friday, December 1.

9:00 A.M. Exhibits of apparatus, second floor of building.

10:00 A.M. General meeting.

Address—'Hypothesis of the Origin of the Earth,' Professor T. C. Chamberlin, head of department of geology, University of Chicago.

Address—'Relation of Forestry to Public School Instruction,' Ernest A. Sterling, U. S. Department of Forestry.

2:00 P.M. Section meetings, seventh floor of the building.

Saturday, December 2.

9:00 A.M. General meeting, reports of committees, general business, election of officers.

10:00 A.M. Section meetings.

1:30 P.M. Excursions by various sections.

PROGRAM—BIOLOGY SECTION.

Friday, December 1, 2:00 P.M.

'What and How Much Can be Done in Ecological and Physiological Zoology in Secondary Schools?' Oscar Riddle, Central High School, St. Louis, Mo.

Report of the committee on 'The Course in Biology in Secondary Schools,' Chairman Worralo Whitney, South Chicago High School.

Discussions based on the committee's report.

'Should Botany and Zoology be Taught in Full Year Courses?' G. H. Bretnall, Monmouth College.

'The Relative Emphasis to be Given to Morphology, Physiology, Ecology and Other Phases of Biology,' Miss Elma Chandler, Elgin High School.

'What, and How Much Field Study may be taught?' Fred. L. Charles, Normal School, De-Kalb, Ill.

'In What Order Should Animal and Plant Groups be Studied?' Miss Amelia McMinn, West Division High School, Milwaukee, Wis.

Open discussion.

Saturday, December 2, 10:00 A.M.

Election of officers and general business of the section.

The laboratory note-book problem, five-minute discussions.

'The Technique of Projection and Anesthesia of Animals, with Numerous Demonstrations,' A. H. Cole, Lake High School, Chicago.

Saturday Afternoon Excursions.—Washington Park Conservatory and Field Columbian Museum, Lincoln Park Conservatory and animal exhibit. Uihlein's orchid houses.

PROGRAM—CHEMISTRY SECTION.

Friday, December 1, 2:00 P.M.

'Recent Advances in Chemical Knowledge,' Dr. L. W. Jones, University of Chicago.

Discussion.

'Equipment of a Modern Chemical Laboratory

in the Secondary School,' Fred. J. Watson, McKinley High School, Chicago.

Discussion.

'Reference Books in Chemistry,' F. C. Irwin, Central High School, Detroit, Mich.; A. B. Crowe, State Normal School, Charleston, Ill.

Saturday, December 2, 10:00 A.M.

Election of officers and general business.

Address—'Investigations on Osmosis,' Dr. Louis Kahlenberg, University of Wisconsin.

Address—'New Theories of Matter in Relation to Chemical and Physical Theory,' Professor Charles T. Knipp, University of Illinois.

PROGRAM—EARTH SCIENCE SECTION.

Friday, December 1, 2:00 P.M.

Address—'Commercial Geography for Secondary Schools,' Dr. J. Paul Goode, University of Chicago.

Address—'The Work of the Hydrographic Office in its Relation to Commerce,' W. J. Wilson, nautical expert in charge of the Chicago Branch of the U. S. Hydrographic Office.

Report of committee on cooperation in gathering materials in teaching physiography, Charles Emerson Peet, chairman, Lewis Institute, Chicago.

Election of officers.

Saturday, December 2.

Excursions.—To the plant of the Automatic Electric Company, thence through a portion of the tunnels of the Illinois Telephone Company. The party will start immediately after the general session of the association, Saturday morning. To the Fat Stock Show and the packing houses at the Union Stock Yards. The party will start immediately after the general session of the association, on Saturday morning. An admission fee of fifty cents will be charged at the gates. Luncheon may be obtained at the yards. To the works of the Pullman Company; this is a joint excursion Saturday afternoon with the Physics Section.

For particulars see program of the Physics Section.

PROGRAM—PHYSICS SECTION.

Friday, December 1, 2:00 P.M.

Address—'The Value of Qualitative Experiment in Physics,' L. F. Miller, University of Wisconsin; 'The Aim of High School Physics Teaching.' E. E. Burns, Medill High School, Chicago.

Report of Committee on 'Reference Books in Physics,' A. H. Sage, State Normal School, Oshkosh, Wis.

Address—'The Teaching of Physics,' H. N. Chute, High School, Ann Arbor, Mich.

Informal Discussion: 'Should the attitude of the student be that of discoverer or verifier?' 'Is consultation of two or more students on a laboratory experiment disorder?' 'Is it certain that some experiments that require several hours to perform are more valuable to a student than the time spent in reading?' 'How many times is it profitable for a student to perform certain experiments for greater accuracy, for example, finding the latent heat of vaporization of water?' 'Must the student record every experiment?' 'Should work with scales and calipers be done in advance for the purpose of learning the use of the instruments, or should the operation be done for the first time when use requires it?'

Presentation of new physical apparatus, Mr. L. B. McMullen, Shortridge High School, Indianapolis.

Saturday, December 2, 10:00 A.M.

Election of officers.

Business.

Joint meeting with Chemistry Section, 10:15 A.M.

Address—'Investigations on Osmosis,' Dr. Louis Kahlenberg, University of Wisconsin.

Address—'New Theories of Matter in Relation to Chemical and Physical Theory,' Professor Charles T. Knipp, University of Illinois.

Saturday afternoon will be devoted to an excursion to the Pullman Palace Car Works, Pullman, Ill. Train will leave Randolph Street Station of the Illinois Central Railroad at 1:20 P.M. Those who wish may visit the Finsen Light Institute of America, Washington Boulevard and Hamlin Avenue.

PROGRAM—MATHEMATICS SECTION.

Friday, December 1, 2:00 P.M.

Address—'The Straight Line in Geometry,' J. W. Withers, principal, Teachers College, St. Louis.

Discussion.

Reports: Willard S. Bass, Francis W. Parker School, Chicago; Miss Mabel Sykes, South Chicago High School, Chicago.

Paper—'Interest and Progress in Teaching Mathematics,' N. J. Lennes, Wendell Phillips High School, Chicago.

Discussion.

Saturday, December 2, 10:00 A. M.

Election of officers.

Address—'Aids in Teaching Algebra,' Professor R. J. Aley, University of Indiana.

Discussion led by Miss Jessie J. Bullock, High School, Champaign, Ill.

Paper—'Some Thoughts on the Teaching of

Geometry,' C. A. Petterson, Jefferson High School, Chicago.

Discussion led by G. C. Shutts, State Normal School, Whitewater, Wis.

O. W. CALDWELL, *President*,
C. M. TURTON, *Secretary*.

THE ONONDAGA ACADEMY OF SCIENCE.

THE regular monthly meeting of the Onondaga Academy of Science was held Friday evening, October 20, 1905, the president, Dr. T. C. Hopkins, in the chair.

Mr. Charles E. Wheelock gave an interesting account of some overthrust faults occurring across central New York from Little Falls to Ithaca and which are most prominently developed in the Scalaris[1] and in the overlying Helderberg limestones. He tried to show that these disturbances were found only in rocks immediately overlying the Salina formations from which the various salts had been leached out. As the rocks of central New York dip slightly toward the south, the hypothenuse of the triangle would be shortened by the dropping down of the overlying formations due to the solution of the salts, and thus produce a lateral pressure in the rocks capable of producing the overthrusts.

Professor Philip F. Schneider read an interesting paper on 'The Correlation of Some Alnoite Dikes in East Canada Creek.' Heretofore but three dikes were known at East Canada Creek, showing only on the Montgomery side, with a narrow dike on the Herkimer side which it was impossible to correlate with either of the others. The paper established the fact that there were five dikes on the east side and also five corresponding dikes on the west side of the stream. All were located accurately and figures given as to their width, distances apart and strike, showing that they were corresponding dikes. Megascopically the dikes bear a close resemblance to those already known and it is believed that a microscopic study of the same would show that they are practically identical.

[1] The Scalaris limestone as described by P. F. Schneider in the October, 1905, number of the *American Journal of Science* is the prominent limestone ledge in the Camillus Shale of the Salina formation and immediately underneath the gypsum deposits. It is the first formation of the salt deposit in central New York containing fossils, the *Leperditia Scalaris* Jones being the most abundant.

Dr. Daniel S. Martin, of Brooklyn, spoke of the close resemblance of the peridotite dikes in New York, Kentucky and South Africa and the possibility of diamonds occurring in them in this country. While the material composing all these dikes is practically identical, as shown by their petrographic study and chemical analyses, even to the extent of their containing certain gems in common, as the pyropes and olivenes, nevertheless the diamond is conspicuous by its presence in the African fields and equally conspicuous by its absence in the American localities.

President T. C. Hopkins spoke of the rumor that two diamonds had been found in the drift deposits south of Syracuse. The owner of the sand bed claimed to have found a good-sized diamond in the drift which was deposited in a Syracuse bank and later sold to a party in Springfield, Mass., for two hundred and fifty dollars. Another so-called diamond obtained from this same sand pit was shown to Dr. Hopkins by the owner, but a hasty examination convinced him that it was a topaz. However, nothing positive was known concerning the character of the first found stone. Geologists were advised to watch carefully excavations, both in the disintegrated dike and in the drift material, for possible diamonds.

PHILIP F. SCHNEIDER,
Secretary.

DISCUSSION AND CORRESPONDENCE.

HONORARY DEGREES.

TO THE EDITOR OF SCIENCE: I have been very much interested in your note in the issue of October 27, concerning the honorary degrees conferred at the recent inaugural of the University of Illinois. Instead of the too prevalent practise of conferring the degree of doctor of laws indiscriminately on all of the gentlemen whom it was desired to recognize, it is pleasing to see the degree of doctor of science given to a gentleman of distinguished scientific attainments, that of doctor of engi-

neering to an accomplished and distinguished engineer as well as to the successful dean of a school of engineering, and that of doctor of agriculture conferred on gentlemen who have done much to promote this great profession.

I should like to call attention to the discussion before the American Society of Naturalists at its St. Louis meeting, as reported in Science of May 27, 1904, in which the question of honorary degrees was analyzed by a number of distinguished speakers, in connection with that of degrees at large. May it not be hoped that the attention of those having it in their power to confer such degrees may be directed once more to the desirability of such differentiation as the University of Illinois has here applied? William Trelease.

SPECIAL ARTICLES.

THE ORIGIN OF BLACK SHEEP IN THE FLOCK.

The phrase 'Every flock has its black sheep' connotes the sporadic nature of their appearance. They crop out in flocks of breeding ewes and rams that are wholly white. When a quality suddenly arises from parents that have its opposite the probability is that the two opposed qualities follow Mendel's law in inheritance and that the new, filial character is recessive, the parental opposite dominant.

There are four tests of recessiveness. First, if the germ gland contains the dominant characteristic that characteristic, and not the recessive, will show in the soma; consequently, the patency of the recessive in the soma of any individual indicates that its germ gland contains only the recessive quality. Hence, when two recessive individuals are interbred they will produce only recessive offspring.

Second, if a recessive individual is mated with a heterogametous individual—*i. e.,* one which because of mixed ancestry, has both dominant and recessive germ cells—fifty per cent. of the offspring should be recessive.

Third, if two heterogametous individuals be mated, twenty-five per cent. of the offspring should, in the long run, be recessive.

Fourth, if recessive individuals (having exclusively recessive germ cells) mate with pure dominant individuals (having exclusively dominant germ cells) the soma of the hybrids must show the dominant characteristic.

An opportunity to test the recessiveness of the black coat in sheep is afforded by the 'Sheep Catalogue' of Dr. Alexander Graham Bell's (1904) flock,[1] giving the records of 877 sheep used or acquired in pedigree breeding by Dr. Bell. We may apply in turn the four criteria.

First, of twenty offspring both of whose parents were black, nineteen were black. When I discovered this fact I wrote to Dr. Bell concerning the exception (No. 814, white, female), and he was good enough to reply:

> I have examined the original entry of birth of 814 and find her reported as wf 4n s born March 23, 1898, out of 712 bf 4n s by 626 bm 5n tw:—still-born, weight 2 pounds. This lamb was still-born and was born in March, this means that I did not see the lamb myself for I am not usually at Cape Breton at that time, and there has not been any verification of color.

Dr. Bell goes on to state that his shepherd has made errors in recording black as white, and *vice versa,* but these "have been corrected by subsequent examination. In this case, as the animal was still-born, the record rests entirely upon the unsupported statement of the shepherd." We may consequently neglect No. 814 and conclude that all descendants of two black parents are black. This result is in accord with the hypothesis that black is recessive.

Second, of 51 offspring of a recessive (black) individual that was heterogametous (because a hybrid between a white and a black parent) 26 were white and 25 black. This accords with the hypothesis that black is recessive.

Third, of 47 offspring, each from two heterogametous parents, 40 were white and 7 black. In every family but one the proportion of blacks is below the 25 per cent. expectation. The result is not in strict accord with Mendelism, although closely allied with it. There is evidently some modifying factor. It may

[1] Bell, Alexander Graham, 1904, 'Sheep Catalogue of Beinn Bhreagh, Victoria Co., Nova Scotia: Showing the Origin of the Multinippled Sheep of Beinn Bhreagh and Giving all the Descendants Down to 1903.' 22 pp. Washington, D. C.

help us if we assume a greater vigor of the white germ cells, so that unions do not take place in hap-hazard fashion, but two germ cells bearing black are less apt to get together than two bearing white, pure black zygotes being produced in less than one fourth of the cases.

We may conclude, then, that while the third criterion of recessiveness is imperfectly met this does not militate against the recessiveness of black in the Mendelian sense, but indicates the presence of a second, disturbing, factor.

The fourth criterion is the least critical because of the impossibility of judging whether a dominant is homogametous or not, except by its performance; if the hybrids are not dominants we conclude that the parent is not a pure dominant. The existence, however, of white individuals which always throw whites when mated with blacks is significant in relation to this criterion. Three white parents, descended, so far as known, from white ancestors, produced, when crossed with black sheep, 13 offspring, all white.

A special case deserves particular mention. No. 907 is a white male both of whose parents, 606 and 810, are also white, but both of whose grandfathers are black. Consequently, 606 and 810 are heterogametous but, until tested, we have no means of knowing whether their son, 907, is heterogametous or has only white germ cells. When No. 907 was crossed with heterogametous, white females all offspring were white. This would indicate that No. 907 is homogametous. When No. 907 was, however, crossed with pure recessives (blacks) one out of five offspring was black, and when crossed with 'extracted' recessives (having one heterogametous white parent) it produced two black offspring out of 18. In relation to these three offspring out of 23, assuming the record to be correct, No. 907 acts as if heterogametous. The occasional appearance of black offspring from a homogametous and a heterogametous parent may be explained as an occasional prepotency of black over the dominant white,—a phenomenon described by Castle[2] (1905, p. 58 et folg.).

[2] Castle, W. E., 1905, 'Heredity of Coat Characters in Guinea-Pigs and Rabbits.' Papers of Station for Experimental Evolution at Cold Spring Harbor, New York, No. 2.

It may be concluded that the fourth criterion also speaks for the recessiveness of black, the only exceptional case being explained on special grounds.

The conclusion of the whole matter is that black wool color in sheep behaves like a Mendelian recessive characteristic.

C. B. DAVENPORT.
DEPARTMENT OF EXPERIMENTAL BIOLOGY,
CARNEGIE INSTITUTION,
STATION FOR EXPERIMENTAL EVOLUTION,
COLD SPRING HARBOR, N. Y.

PHOTOTROPISM IN THE LARVAL AND EARLY ADOLESCENT STAGES OF HOMARUS AMERICANUS.

IN view of the interest which, by the recent excellent work of Keeble and Gamble on the color physiology of higher crustacea, has been renewed in the study of the effects of light upon many forms of littoral crustacea, the following results obtained during the past summer, in a series of experiments upon the effect of light on the larval and early adolescent stages of the American lobster, may be appreciated by some who are engaged in investigation of a similar nature upon other forms. The records of the following experiments cover but a small part of the field of inquiry into the effect of light upon these forms, in so much as they do not take up the subject of the influence of light upon chromatophore activity or pigment movement, but merely attempt to describe the reactions of the first five stages of *Homarus americanus* to light, upon backgrounds of black and white.

The apparatus used for the experiments consisted of an oblong wooden box whose inside dimensions were 6 x 18 x 4 inches (deep). The box was black inside and fitted with a light-tight cover, through one end of which protruded, to a length of 6 inches, a cardboard tube, 1½ inches in diameter, the function of this tube being to admit none but nearly parallel rays of light into one end of the box, thus distinctly localizing the light area. In cases wherein a white background was required, the black interior of the box was merely covered with white paper, as was the inside of the light-tight cover.[1]

[1] The design of the box is based upon that of Keeble and Gamble.

The method of conducting a single experiment was essentially as follows: The box was filled with salt water to a depth of 2 inches or thereabouts, and placed in a quiet and level position. The desired number of lobsters, together with sufficient salt water to make the total depth about 3 inches were added. When the water had quieted and the young lobsters had arranged themselves more or less uniformly in the water area, the cover was placed in position. At intervals, varying from 5 to 15 minutes, the cover was removed and the position of the young lobsters was observed. After some of the observations the cover would be reversed in position, so that the illuminated area in the water would be changed to the opposite end of the box. After other observations, however, the cover would be left as in the first instance, or removed entirely until another uniform distribution in the position of the young lobsters had been obtained. Whether the position of the cover was changed or not, the results, with few exceptions, agreed with great uniformity.

The light intensity was regulated by the time of day at which the observations were made—either at noon, mid-afternoon or nearly evening. In this way, without using artificial means, it was possible to regulate the degree of light intensity with a very fair degree of precision. In the case of studying monochromatic lights (red and blue) sheets of glass were used, the plate being placed over the entrance of the tube. Without doubt, liquid filters would have been an advantage; but the experiments which were made with glass plates gave such marked and definite reactions that they were judged satisfactory for preliminary work.

The counts were made by dividing the field into three areas, namely, the illuminated, the mid-area and the dark. Owing to the fact, however, that the illumination in the mid-area must have been almost imperceptible, for practical results it might have been quite safe to include the mid-area counts with those of the dark area, but for sake of surety they have been considered as a separate area. In the greater number of cases it was an easy matter to count the number of individuals in each of the three areas before a change in position took place. The number of twenty individuals was, in most cases, used for experimentation, for so small a number distributed over three areas could be taken in at a glance, and furthermore twenty seemed a sufficient number to give representative results. In the following account are recorded experiments carried on with the first five stages of *Homarus*. All the conditions of light and backgrounds were not brought to bear upon all five stages, and only a sufficient number of experimentation reports are here recorded to show the general drift of the results.

EXPERIMENT I.

Conditions: black background; sunlight dull; 20 first stage larvæ.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	2	6	12	
2	3	4	13	Reversed.
3	1	5	14	
4	3	4	13	Reversed.

Similar results were obtained when a greater intensity of light was used.

EXPERIMENT II.

Conditions: white background; sunlight dull; 20 individuals used, first stage.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	2	5	13	
2	2	2	16	Reversed.
3	2	3	15	Reversed.
4	1	5	14	

EXPERIMENT III.

Conditions: white background; sunlight bright; 30 individuals used, first stage.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	22	4	4	
2	18	7	5	Reversed.
3	20	7	3	

These results seem to show that the first larval stage of *Homarus* is negatively phototropic on a black background, with both dull and bright light; but that while on white backgrounds he is, under low intensity, negatively phototropic, if the intensity of light becomes greater he becomes positively phototropic. Similar results were obtained with

second and third stage larvæ under similar conditions of light and background.

EXPERIMENT IV.

Conditions: white background; red monochromatic light; 20 first stage larvæ.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	2	1	17	
2	2	3	15	
3	3	4	13	Reversed.
4	6	2	12	

EXPERIMENT V.

Conditions: white background; blue monochromatic light; 20 first stage larvæ.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	12	4	4	
2	11	3	6	Reversed.
3	13	2	5	

These two experiments indicate that in the case of a white background and red monochromatic light, the first stage lobsters are negatively phototropic, while in the case of a white background and a blue monochromatic light the same lobsters are positively phototropic. This was naturally somewhat unexpected, but in all the experiments involving similar conditions of light and background, the second and third stage lobsters respond in a similar manner. In case, however, a black background is used with lobsters of the first three stages, numerous experiments demonstrate a negative phototropism under the conditions of both red and blue light.

EXPERIMENT VI.

Conditions: black background; 20 early fourth stage lobsters; sunlight bright.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	13	3	4	
2	15	3	2	Reversed.
3	12	4	4	
4	15	4	1	

When experiments were tried with the fourth stage, however, a different reaction was found to occur. On black backgrounds and with lights of any intensity or color, the fourth stage lobsters appeared, contrary to the first three stages, positively phototropic, as the following table demonstrates. The degree of light intensity made no further change in the results, save that in instances where the light was the least intense the reaction was least marked; and when the light was most intense, as obtained by reflecting rays of light by means of a mirror into the tube, the definiteness of reaction was most evident.

When white backgrounds were used in connection with the fourth stage lobsters, it was found that in every case except with the monochromatic red light, a positive phototropic reaction resulted. The latter, which was also contrary to expectations, may be outlined as follows:

EXPERIMENT VII.

Conditions: white background; monochromatic red light; 20 early fourth stage lobsters.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	8	5	6	
2	1	6	13	Reversed.
3	1	10	9	Reversed.
4	4	5	11	

These resulting reactions in the case of the early fourth stage lobster may offer an explanation for the fact that this stage is so frequently caught in tow-nets drawn over the surface of any of our shore waters, while it has been a very unusual occurrence to secure in this manner either the earlier or the later stages. The same causes may also have worth for the reported facts that certain stages of the free-swimming larvæ of other forms of crustacea are found more frequently at the surface than are other larval stages of the same species.

It was a noteworthy fact, however, that old fourth stage lobsters would never manifest positive phototropic reactions with the same degree of certainty as that demonstrated in the case of younger fourth stage lobsters. Indeed, in a number of instances, fourth stage individuals which were due to molt within a period of one or two days, manifested on black backgrounds a definite tendency towards a negative phototropic reaction. This response was assumed without exception after the lobsters had molted into the fifth stage, and this reaction was manifested with any combination of light intensity, color or background. The following tables show the reac-

tions in the case of the late fourth and the fifth stage lobsters.

EXPERIMENT VIII.

Conditions: black background (similar results were seldom obtained on a white background); sunlight bright; 20 late fourth stage lobsters.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	9	4	7	
2	11	5	4	Reversed.
3	7	6	7	
4	9	3	8	

EXPERIMENT IX.

Conditions: black background; medium sunlight; 12 fifth stage lobsters.

Test.	Lt. End.	Mid-area.	Dk. End.	Cover.
1	2	4	6	
2	2	3	7	
3	2	2	8	Reversed.
4	1	3	8	

The results of these experiments may also explain, to a certain degree, the facts which appear through the observation of large numbers of the larval stages of *Homarus* when confined and exposed to different light conditions, as they may also interpret to some extent the behavior observed in the larval and early adolescent stages of lobsters under natural conditions of environment. The first three larval stages, when confined in the large twelve-foot white canvas bags in which they were observed, manifested at all times a marked tendency to sink toward the bottom—except perchance at night, when more active swimming is observed in all the stages. This tendency during the daytime could not be controlled in any way. At night, however, it was possible to evoke a seemingly positive phototropic reaction from any of the thousands of young larvæ in the large canvas bags. This was accomplished by means of an acetylene light so directed against a certain area of the white field of canvas that large numbers would at once group themselves thickly about the illuminated area, manifesting, in the case of the third and fourth stages, such an effort to come into the light area that they would often throw themselves partially out of water, causing thereby numerous surface ripples. Since, however, similar results could be obtained when a black background was employed with the acetylene rays, and since the results were not so definite when the incident rays struck the water perpendicularly as when they were thrown at an angle, it was assumed that these reactions were not true phototropisms, but were largely due to the effort on the part of the young lobsters to move in the direction of the incident light rays. This phenomenon was better observable in the fourth stage of *Homarus*, when the very definite rheotropic proclivity, first clearly observable in this stage, could be entirely broken up by introducing the incident rays either at right-angles to or in opposition to the direction of the current. The fourth stage lobsters, however, even under the natural conditions of light, swim actively at the surface. It is not until the fifth stage that the bottom-seeking and 'hiding-habit' is fully established.

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BROWN UNIVERSITY,
October 10, 1905.

AN ILLUSTRATION OF THE USE OF THE WIRE-BASKET METHOD FOR SOIL TESTING.[1]

THE method of cultures in paraffined wire baskets, for determining the relative agricultural values of soils and for investigating the effects of various fertilizers, which was described in Bulletin No. 23 of this bureau, consists in growing wheat seedlings in the soils to be tested for from two to three weeks, determination being made of the water lost by transpiration and of the green and dry weights of the plants at the end of the period. Where differences between the various treatments are developed it is found that the transpiration varies with the weight, being, therefore, a fair measure of growth.[2] This method, which virtually furnishes a pot the walls of which are composed of soil cemented with paraffin, causes a uniform distribution of roots in the soil and exhibits the effects of

[1] By permission of the Secretary of Agriculture.

[2] See in this regard a paper on the relation of transpiration to growth in wheat, about to appear in the *Botanical Gazette*.

various soils upon growth much more clearly than do cultures in ordinary pots, where most of the roots come to lie between the soil surface and that of the pot.

The soil of the present illustration is of the Orangeburg clay type, from South Carolina. Another sample of the same type from Texas is used for comparison. The soil is generally very fertile in both states. To investigate the effects of fertilizers upon the sample from South Carolina, wheat cultures in paraffined wire baskets were grown for three weeks in this soil with various treatments. By this means the surprising discovery was made that the untreated soil gave an exceedingly poor growth of the wheat plants, and that it was not appreciably benefited by any one of a large number of treatments used. An experiment was then carried out to compare this soil with the Texas sample of the same type and with Takoma lawn soil (see Bulletin No. 23 of this bureau), one of the poorest soils with which the bureau has dealt. Transpiration data from thirty plants for twelve days showed that, considering the transpiration on Takoma soil as 100, that on the South Carolina sample was 41, while that on the Texas sample was 209. On the same basis the green weights of the plant tops grown on the two soils were 35 and 216, respectively. Thus by the basket method it appears that this particular sample from South Carolina is exceedingly unproductive, and that the unproductivity is not corrected by fertilizers.

It seemed possible that the observed sterility might be due to the presence of toxic organic substances in the soil. Such toxic substances have been found to be readily transmitted to the soil extract and to show their characteristic effects therein, and so to determine whether or not such substances were present here wheat seedlings were grown in aqueous extracts of the two soils from South Carolina and Texas. These extracts were prepared as described in Bulletin No. 23, by stirring for three minutes five parts by weight of soil with six parts of water, allowing the mixture to stand twenty minutes and then filtering off the extract through a Pasteur-Chamberland filter tube. The plants of the present experiment showed somewhat better growth in the Texas extract, but the difference was not marked. The transpirations from the two cultures of sixteen seedlings each, grown sixteen days, were, for the South Carolina sample 118.4 g. and 117.1 g., and for the Texas sample 148.8 g. and 129.3 g., respectively. This indicates that the soil to be tested contains no soluble organic substances markedly toxic to the plants and that it does contain sufficient soluble inorganic material for normal plant growth. Its infertility must, therefore, be due either to some physical property of the soil or to too great a concentration of the soluble salts in the soil. The effect of such concentration might be overcome to a very large extent by the much greater dilution of the soil extract.

To determine whether the infertility might be due to too great concentration of soluble matter, the South Carolina sample was leached by passing about an equal volume of water through it, and the leached sample was compared with the Takoma soil by another basket experiment which ran for six days. Considering the transpiration and green weight each as 100 for the Takoma soil, the figures obtained from the leached soil were 196 and 152, respectively. Thus by leaching the soil its power to support plant growth was increased from 41 to 196 by transpiration and from 35 to 152 by green weight. On the same basis, the fertility of the Texas sample is represented by 209 for transpiration and 216 for green weight, so that by leaching the sterile sample it has been improved in fertility so as to be nearly equal to the Texas sample. These results show that the sterility of the South Carolina sample, at least as far as seedling wheat plants are concerned, is probably due to an excess of soluble salts. Chemical analysis of the water extract of the South Carolina sample showed the following amounts of dissolved materials, expressed in parts per million of the air dry soil by weight: NO_3, 611; PO_4, trace; SO_4, trace; K, 100; Ca, 11; and Cl, 175. The large amounts of nitrate, chloride and potassium here found seem to cor-

roborate the conclusion reached by the method of basket cultures.

To test this proposition still further, the baskets of untreated soil used in the first fertilizer test were replanted and the plants allowed to grow three weeks. The result showed the soil to have improved to the extent of about 183 per cent. by transpiration and 86 per cent. by green weight. This observation suggested that possibly the plants of the first planting had absorbed from the soil sufficient salts to reduce the concentration of the soil solution to a considerable degree, although the plants had made but a poor growth, and that in this way the injurious property of the original sample had been largely corrected. At the end of this second culture the soil was again subjected to a chemical analysis of its water extract, with results which showed clearly that the above explanation is the correct one. The following amounts of dissolved materials, expressed as before in parts per million of air dry soil, were found: NO_3, 87; PO_4, trace; K, 29; Ca, 4; and Cl, 100. It is obvious that a marked decrease in dissolved salts has indeed taken place.

While the last test was in progress basket cultures were carried on with three new samples of this soil from other spots in the same field, these having been obtained in order to determine whether or not the first sample was typical of the whole field. The average growth in the three new samples was very much better than that in the first planting of the original samples, the difference amounting to 322 per cent. by transpiration and 110 per cent. by green weight. Thus it became apparent that the original soil sample was not typical of the field from which it was taken and that in general the field is not unproductive.

It appears then that the particular spot from which the original sample was taken has in some way, possibly by over-fertilization, too high a soluble salt content for good plant growth.

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OBSERVATIONS ON COLOR PERCEPTION AMONG THE VISAYANS OF LEYTE ISLAND, P. I.

As a United States government teacher in the public schools of Leyte Island, I became interested in the dialect of the Visayans. In this study I found frequently suggested what sort of men the Visayans were when the Spaniards came to their islands. The modified Spanish words in the vocabulary of the present native designate ideas given to the Visayans by the Spaniards. It was not long before I was confronted with the same question that Gladstone encountered in his study of Greek. Gladstone by pointing out that in the Homeric vocabulary there were no words for blue, and by concluding that in the time of Homer the Greeks did not see blue, opened quite a controversy over the evolution of color perception as based upon the nomenclatures of ancient people and savages of the present. Geiger advanced the theory that red was the first color seen by man and after that the other colors in their order as formed by the spectrum. Geiger was supported by Magnus with further philological evidence. This discussion until recently had been considered closed. Havelock Ellis in Vol. 69 of *The Contemporary Review* at page 715 says: "There is no doubt whatever that all races of men, concerning whom any evidence can be obtained, have been acquainted with the same regions of the spectrum we have known." After so strong a statement I was surprised to find Rivers, who had made extensive experiments upon the Papuans of Torres Straits, saying of his work that one of its chief interests 'is that it shows a defect in nomenclature for a color may be associated with defective sensibility for that color and so far lends support to the views of Gladstone and Geiger.'[1] The evidence that I have obtained among the Visayans also supports the views of Gladstone and Geiger. My discovery that the people had no words for the colors higher than yellow was new to me, but I later learned that this fact was known as early as 1869. Words for the higher colors which are used to-day by the natives are bor-

[1] 'Reports of the Cambridge Anthropological Expedition,' Cambridge, 1901, Vol. II., Part I., p. 49.

rowed Spanish terms. I found *siga, meserum, mabosag, maitum, mapula* and *madarag* were fixed words for *light, darkness, white, black, red* and *yellow,* respectively. I tried all methods suggested to find pure Visayan words designating the colors of more rapid rates of oscillations. All classes of the people were consulted, but no such words were brought to light.

The most comprehensive dictionary of the Visayan as spoken by the people of Leyte is the large 'Diccionario Hispanio-Bisaya; Bisaya-Hispanio,' by R. P. Fr. Antonio Sanchez. From it the following was taken:

Spanish.	Visaya.	English.
Verde.	Lunhao, hilao, hayat, banua.	Green.
de Café.	de Café.	Brown.
Purpuro.	Purpuro.	Purple.

Here we see given four Visayan words for green. None of these, however, is actually used by the natives for designating green. So we turned to the Bisaya-Hispanio part of the dictionary and found the following:

Visaya.	Spanish.	English.
Lunhao.	Color verde.	Color green.
Hilao.	Cosa cruda; verde.	Thing crude; green.
Hayat.	Cosa verde; sin maduraion.	Thing green; without maturity.

It will be seen here that all the words used for the definition of the Spanish verde express rather the general condition common to things not dried, things unripe and the general appearance of the grass, fields and forests —*lunhao* excepted. For six months I carried *lunhao* about with me. I could find but one man to recognize this word. El Capitan Louis Cordero, an intelligent citizen of Burauen, explained to me that it did not express the idea of verde or green, but that it also had reference to an uncured state. He gave as an example the condition of nipa thatching before it was completely dried and ready for use. *Lunhao,* therefore, is an exceedingly doubtful word by which to express the idea of green. In fact my efforts led me to conclude that it does not have any such meaning. But be that as it may it is quite a suggestive fact that so much vagueness hangs about the idea of green as expressed in truly Visayan words.

And when we come to the color next higher in the scale of the spectrum we find no Visayan word whatever given for it. The dictionaries give the Spanish *azul* modified into *asul.* And this is the only word the natives use. For violet and other colors nothing but pure Spanish words are used. So far as the present dialect is concerned we are led to infer that until the Spaniards came the Visayans had no ideas of green, blue and violet, which demanded words for their expression.

But not satisfied with these observations, I turned to the children of the island. Children, of course, can not be expected to handle colors as well as adults. But with this caution in mind some results were obtained which, because of their lack of variation are of considerable interest.

In speaking with the American teachers I learned that they experienced difficulty in getting the children to recognize colors. Mr. B. M. Sullivan, an American teacher at Dagami, pointed out to me the fact that 'the ladies nearly always wear red and yellow. When they do wear dark blue they call it black. Their light blue skirts they call green.'

On February 15, 1902, a purple kite sailed by my window. This a boy servant, twelve years old, pronounced de café—brown. Later he called the same kite red. On March 27, 1902, another servant, seventeen years old, bought a pair of purple slippers, which he said were brown.

On March 14, 1902, while I was sitting with Mr. Sullivan, six little girls came into his room. They were about nine years old. These little girls had never seen the green hyloplate writing boards which the educational department had recently sent. The girls were soon before them eagerly disputing their color, for they had expected writing boards to be black. Were they green, blue or black was a difficult question for them. After much dis-

cussion and reading of our faces they finally decided that they were green.

Several days later we took the matter of color perception into the Dagami schools. Mr. Sullivan, their teacher, asked a nine-year-old girl the color of a handkerchief that she wore about her neck. She answered correctly that it was blue. But to his next question she replied that the grass was the same color. Another girl about ten years old pronounced a rather dark green leaf *maitum*—black. A class of thirteen girls in the same school, ranging in age from eight to twelve years, named properly and without any difficulty the colors of pencils painted red and yellow. But the green and blue pencils they could not at all name. They simply could not perceive the green and blue as such.

On September 9, 1902, in dealing with a class nine to ten years or older, who had had no chance either in Spanish or American times to improve themselves, I found them very ready to recognize red and yellow. All seven would respond with the proper Visayan words for these colors. One of the class wore dark blue trousers; they were decidedly blue, yet not as dark as navy blue. For the color of these trousers six of the boys gave the Visayan word *maitum*—black—and the seventh boy, to get ahead of his classmates, shouted out the Spanish word *negro*—black. The same class had no difficulty with the red and yellow as given in the color chart of E. G. Regal's 'Lessons for Little Readers,' Heath and Company. But I found they as readily called light blue green or brown as anything, and no better did they handle the green.

Mr. H. E. Guyer, one of the American teachers at Tacloban, gave me the following statement:

> The dullest boy in my school has never called a red ball anything but a red ball. Except for a thoughtless boy's answer they invariably call a yellow ball yellow. In other colors I can not rely upon them, except some few who have memorized them. And even they flounder if I mix the colors up.

It has been suggested that these observations may be worthless on the ground that any children might make similar mistakes in naming colors. I can not think that these observations can be explained away in this manner, because, in the first place, if this were the cause the colors mistaken would not have been so constantly green and blue. And, secondly, this differs radically from results of experiments made upon children of parents who have a well-defined perception of blue and green. Marx Lobsien in a series of experiments upon German school children observed that the three fundamental colors, *Rot, Gelb* und *Blau,* were almost accurately handled. Of these blue was more accurately handled than yellow. "Nach meinen Untersuchungen steht am höchsten in der Wertung da das Rot. Es wurde auf allen Alterstufen immer richtig aufgefasst und benannt; ihm fast gleich, nur auf der fünften Stufe findet sich eine kleine Unterschwankung, ist das Blau, dann folgen 3 Gelb, 4 Grün, während Orange, Violette, Indigo unverhältnissmässig ungünstig dastehen."[2]

It has also been explained that primitive men have no words for green and blue because they have no interest in these colors. Rivers found that the Paupan name for red was the modified term used to designate the female parrot *Eclectus polychlorus.* On the island of Leyte there is a dark blue snake with a yellow head which the natives fear greatly because of its poisonous fangs. If so inoffensive an animal as a parrot could have attracted the early Paupans and given them their name for red, one marvels that the early Visayans were not attracted by this snake to blue. I have no doubt that other instances could be found in which green and blue were strikingly presented, so that the pre-Spanish Visayans would have taken notice of them if they had seen these colors as such.

These observations, therefore, lend weight to the theory that the eye, in its evolution as a color-sensing organ, saw at first only the colors at the red end of the spectrum; and seem to show that the Visayans, at least at the time when they were discovered by the

[2] *Zeitsch. f. Psych. und Phys., etc.,* Bd. XXXIV., Heft I., January 19, 1904, p. 35.

Spanish, were still in the stage when red and yellow were the only colors clearly perceived.[3]

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THE NATIONAL ACADEMY OF SCIENCES.

THE academy held its autumn meeting at the Sheffield Scientific School of Yale University, on November 14 and 15. The scientific program was as follows:

JOHN TROWBRIDGE: 'Slow movements of electrical discharges.'

E. B. WILSON: 'Sex-determination and the chromosomes.'

L. B. MENDEL: 'Studies on the chemical physiology of development and growth.' (Introduced by R. H. Chittenden.)

W. M. DAVIS: 'The Dwyka glacial conglomerate of South Africa.' (Illustrated by lantern slides.)

B. B. BOLTWOOD: 'The disintegration products of thorium as indicated by the proportions of lead and helium in minerals.' (Introduced by H. L. Wells.)

A. HALL: 'Relation of the true anomalies in a parabola and a very eccentric ellipse having the same perihelion distance.'

S. L. PENFIELD: 'On a new mineral from Borax Lake, California.'

F. E. BEACH: 'On errors of excentricity and collimation in the human eye.' (Introduced by C. S. Hastings.)

C. S. PEIRCE: 'The relation of betweenness and Royce's O-collections.'

L. P. WHEELER: 'Some problems in metallic reflection.' (Introduced by C. S. Hastings.)

FRANZ BOAS: 'On Pearson's formulas of skew distribution of variates.'

A. AGASSIZ: 'On the variation in the spines of sea urchins.'

W. H. BREWER: 'Further observations on sedimentation.'

H. A. BUMSTEAD: 'The effect of Röntgen rays on certain metals.' (Introduced by C. S. Hastings.)

THE GEOLOGICAL SOCIETY OF AMERICA.

THE eighteenth annual meeting of the Geological Society of America will be held on December 27, 28 and 29, in Ottawa City, the Canadian capital. The circular of information issued by Secretary Fairchild gives the details of arrangements for the meeting there, and facilities regarding customs as well as railway and hotel accommodation usually given to the fellows of the society.

Ottawa is easily reached from all railway centers and is one of the most progressive cities of the Dominion, being the seat of government and the headquarters of the Geological Survey Department. This official survey, which began in 1842, has continued its operations uninterruptedly and there is now attached to the department a Museum of Geology, for petrography, general geology and historical geology, as well as for paleontology. Type specimens of Canadian fossils, described by Billings, by Whiteaves, Sir William Dawson, by Rupert Jones and by various other well-known authors, are deposited in the collections and can be seen to advantage.

A large attendance is expected at this meeting, many having already signified their intention of being present. A local committee has charge of the details of the meeting, and the evening sessions promise to be of an interesting nature. The annual dinner of the society will be followed by a reception at which the governor general will be present. The Russell House will be the headquarters. Rates are very reasonable, and every comfort will be provided for the guests attending the meeting. All parcels serving to illustrate papers to be presented at the meeting will be admitted free of duty by the commissioner of customs if addressed to Dr. H. M. Ami, Headquarters of the Geological Society of America, Russell House, Ottawa, Canada.

The society met at Ottawa in 1892 under the presidency of Professor B. K. Emerson. This year Professor Raphael Pumpelly is the president.

THE ROYAL SOCIETY'S MEDALS.

THE following is a list of those to whom the Royal Society has this year awarded medals:

The Copley medal to Professor Dmitri Ivanovitch Mendeléef, of St. Petersburg, for

[3] I am indebted to Professor R. S. Woodworth, of Columbia University, for valuable suggestions and references to literature. He is not, however, responsible for the conclusions advocated.

his contributions to chemical and physical science.

A Royal medal to Professor John Henry Poynting, F.R.S., for his researches in physical science, especially in connection with the constant of gravitation and the theories of electrodynamics and radiation.

A Royal medal to Professor Charles Scott Sherrington, F.R.S., for his researches on the central nervous system, especially in relation to reflex action.

The Davy medal to Professor Albert Ladenburg, of Breslau, for his researches in organic chemistry, especially in connection with the synthesis of natural alkaloids.

The Hughes medal to Professor Augusto Righi, of Bologna, on the ground of his experimental researches in electrical science.

MEETING OF TRUSTEES OF THE CARNEGIE FOUNDATION.

The first meeting of the trustees of the Carnegie Foundation, the $10,000,000 fund given by Mr. Andrew Carnegie last May for the pensioning of college professors, was held, on November 15, at Mr. Carnegie's residence, in New York.

The morning session was devoted to an informal conference. After luncheon Mr. Carnegie called the meeting formally to order. In a brief speech he expressed satisfaction at gathering together so many prominent educators. Nothing he had ever done, he said, seemed so propitious or so likely to be useful to the cause of education as this gift. He expressed the hope, in conclusion, that the trust would be administered in a broad and generous manner. President Eliot, of Harvard, in reply expressed the thanks of the teaching profession to the donor.

The board of directors organized by electing President Eliot chairman, President Harper vice-chairman and President Thwing secretary. By-laws were then adopted providing that the business of the board be entrusted to an executive committee and a president who shall be chairman of the committee. Dr. Henry S. Pritchett, president of the Massachusetts Institute of Technology, was chosen president. The committee was made up as follows: President Butler, of Columbia; President Wilson, of Princeton; Provost Harrison, of the University of Pennsylvania; President Humphreys, of Stevens Institute; Mr. Vanderlip, vice-president of the National City Bank; and Mr. Franks, Mr. Carnegie's financial secretary.

It was decided that the head office of the foundation shall be in New York City. The third Wednesday in November was selected as the date of the annual meeting. A special meeting will be held in New York late in the winter, at which time the executive committee will make a report on the plan and scope of the organization.

All the trustees except President Harper, of Chicago, were present, namely: Charles W. Eliot, Harvard; A. T. Hadley, Yale; Nicholas Murray Butler, Columbia; Woodrow Wilson, Princeton; Jacob G. Schurman, Cornell; L. Clark Seeley, Smith; Charles C. Harrison, Pennsylvania; Alexander C. Humphreys, Stevens Institute; S. B. McCormick, Western University of Pennsylvania; Edwin B. Craighead, Tulane; H. C. King, Oberlin; Edwin H. Hughes, De Pauw; C. F. Thwing, Western Reserve University; Thomas McClelland Bell, Drake; George H. Denny, Washington and Lee; Principal Peterson, McGill University, Montreal; Samuel Plautz, Lawrence; David Starr Jordan, Stanford; W. H. Crawford, Allegheny; Henry S. Pritchett, Massachusetts Institute of Technology; F. A. Vanderlip, T. Morris Carnegie and Robert A. Franks.

SCIENTIFIC NOTES AND NEWS.

Professor Albert von Köllicker, the eminent anatomist and zoologist, died at Würzburg, on November 3, at the age of eighty-eight years.

At the meeting of the Academy of Natural Sciences of Philadelphia, held on November 7, the Hayden memorial gold medal was unanimously voted to Charles Doolittle Walcott, director of the United States Geological Survey, in recognition of the value of his individual contributions to geological science and of the benefits derived from his able and con-

scientious discharge of the official trust confided to him.

THE daily papers announce that the Mexican Astronomical Society has awarded the prize offered by the Bishop of Leon for some notable astronomical discovery to William H. Pickering, of Harvard College Observatory, for the discovery of the tenth satellite of Saturn. Another prize was awarded to Professor C. D. Perrine, of Lick Observatory, California, for the discovery of the sixth and seventh satellites.

THE French Academy of Moral and Political Sciences has decided to award the François-Joseph Audiffred prize, of the value of $3,000, which is given in recompense of the most beautiful and greatest acts of self-devotion of whatever kind they may be, to Professor Calmette, director of the Pasteur Institute at Lille.

MEDICAL journals state that medals were awarded by the recent Congress of Tuberculosis to Drs. Koch, of Berlin; Brouardel, of Paris; Bang, of Copenhagen; Biggs, of New York; Broadbent, of London; and von Schroetter, of Vienna.

PRESIDENT ELIOT, of Harvard University, gave, on November 13, the first lecture under the foundation recently given to Yale University by a Harvard alumnus 'to promote friendly feelings between the two universities.' Dr. Eliot spoke on 'Resemblances and Differences among American Universities.'

DR. LEWELLYS F. BARKER, professor of medicine in the Johns Hopkins University, delivered an address before the Library and Historical Society of the University of Maryland, Baltimore, October 26, on 'The Ordering of Life.'

PROFESSOR K. KLEIN has been appointed director of the Museum of Natural History of the University of Berlin, in which position he succeeds Dr. Karl Möbius.

MR. G. W. SMITH, New College, Oxford, has been appointed to the biological scholarship at Naples for the year 1905–6.

DR. MAURITS SNELLIN has resigned the directorship of the section of terrestrial magnetism and seismology at the Royal Dutch Meteorological Institute.

SIR FREDERICK TREVES, the well-known surgeon, has agreed to be nominated on nonpolitical and academic grounds for the lord rectorship of Aberdeen University.

DR. JOHN DYNELEY PRINCE, professor of Semitic languages and literature in Columbia University, has been elected to the New Jersey State Assembly from Passaic County.

WE learn from the *British Medical Journal* that King Edward on the occasion of his birthday has conferred the honor of knighthood upon Dr. James Barr, physician to the Royal Infirmary, Liverpool, and lecturer on clinical medicine in University College, Liverpool, and upon Mr. Arthur Chance, president of the Royal College of Surgeons in Ireland. Dr. Theodore Thomson, one of the medical inspectors of the Local Government Board, has been made a C.M.G. in recognition of services rendered in connection with sanitary matters to the Foreign Office and Colonial Office. The same honor is conferred upon Dr. Marc Armand Ruffer, president of the Egyptian Sanitary Board, distinguished for his researches in bacteriology and pathology; and upon Dr. Featherston Cargill, resident in the Protectorate of Northern Nigeria. Sir Felix Semon, C.V.O., physician extraordinary to the king, has been advanced to be a knight of the Royal Victorian Order. The honor of knighthood has also been conferred upon Mr. John McFadyean, professor of comparative pathology and bacteriology, Royal Veterinary College, London.

AT a recent meeting of the directors of the Christian Association of the University of Pennsylvania it was resolved to send Dr. Josiah C. McCracken to China for a period of one year in order to study the situation on the field and arrange details for the establishment of the proposed medical school in Canton.

DR. C. H. GORDON, until recently professor of geology in the New Mexico School of Mines, has been engaged for several months in the study of the geology and ore occur-

rences of the Magdalena and Black Range districts of New Mexico for the U. S. Geological Survey.

PROFESSOR G. F. HULL, of Dartmouth College, is spending the year at the Cavendish Laboratory, Cambridge. He has been elected fellow commoner by the council of St. John's College.

DR. SVEN HEDIN is on the way to Persia, where he proposes to explore thoroughly, from a scientific point of view, the salt deserts of Dasht-i-Kavir and Dasht-i-Lut in the eastern part of the country. He hopes afterwards to proceed through Afghanistan to India, and there organize an expedition for the exploration of Central Tibet.

Nature states that at the inaugural meeting of the eighty-seventh session of the Institution of Civil Engineers, held on Tuesday, November 7, Sir Guilford Molesworth, K.C.I.E., the retiring president, formally introduced to the members his successor in the chair, Sir Alexander Binnie, who delivered an address to the members, in which he traced the influence of scientific thought and investigation upon the development of engineering practise.

SIR ARCHIBALD GEIKIE expected, on behalf of the board of geographical studies, of Cambridge University, to deliver a public lecture in the Sedgwick Museum, on November 21, on 'The Evolution of a Landscape.'

PROFESSOR GEORGE M. STRATTON, of the Johns Hopkins University, lectured last week before the department of philosophy, of Wesleyan University, on 'Optimism and the Scientific Method.'

A PUBLIC meeting on the mosquito question was held at the Johns Hopkins University, Baltimore, on November 1, at which Professor John B. Smith, state entomologist of New Jersey, delivered an illustrated lecture.

AN oil portrait of Dr. John Rodman Coxe, was presented, by his grandson, to the department of medicine of the University of Pennsylvania, on October 20. In 1809 Dr. Coxe was appointed professor of chemistry in the University of Pennsylvania, and from 1818 to 1835 he was professor of materia medica and pharmacy. He was a trustee of the university and one of the founders of the Philadelphia College of Pharmacy.

DR. F. A. MÜLLER, formerly professor in the veterinary institute of the University of Vienna, died on October 16 at the age of eighty-nine years.

THE death is also announced of Professor Albert Engelbrecht, of the Government Laboratory for Chemistry, at Hamburg.

WE learn from *Nature* that at a meeting of the council of the British Association on November 3, it was decided that, in consequence of representations by the local committee, the meeting at York next year shall be opened on Wednesday, August 1, which is earlier than the usual date. The council of the association has received a gift of £50 from Mrs. John Hopkinson, to be devoted to some investigation which may be suggested at the next meeting by the committee of recommendations.

THE fourth meeting of the California Branch of the American Folk-Lore Society was held at the University of California in Berkeley, on Tuesday, November 14. Professor John Fryer delivered a lecture on 'Fox-Myths in Chinese Folk-Lore,' illustrated by specially prepared lantern slides.

THERE will be a New York State civil service examination on December 9, when positions will be filled of assistant chemist in the Cancer Laboratory at Buffalo, with a salary of $720; for physician in the State Hospitals, beginning at an annual salary of $900 and increasing to $1,200 with maintenance, and of assistant statistician in the Department of Excise, with a salary of $1,500 to $1,800.

A TELEGRAM has been received at the Scottish National Antarctic offices in Edinburgh from Mr. Angus Rankin, announcing the arrival of the Ben Nevis Observatory staff at Buenos Ayres, from which place they leave shortly for the Antarctic station at Scotia Bay, South Orkneys.

THE British registrar-general has issued his returns relating to the births and deaths in the

third quarter of the year. According to the abstract in the *British Medical Journal,* the births registered in England and Wales during the quarter ending September last numbered 235,205, and were equal to an annual rate of 27.3 per 1,000 of the population, estimated at 34,152,977 persons in the middle of the year. This is the lowest birth rate recorded in the third quarter of any year since civil registration was established, and was 1.8 per 1,000 below the average in the corresponding quarters of the ten preceding years, 1895–1904. Among the several registration counties the birth rate last quarter ranged from 21.3 in Sussex, 22.6 in Herefordshire, 22.8 in Dorsetshire and in Cornwall, and 22.9 in Somersetshire, to 30.8 in Nottinghamshire, 32.2 in Northumberland, 32.6 in Carmarthenshire, 34.1 in Durham, 34.6 in Glamorganshire, and 35.7 in Monmouthshire. In seventy-six of the largest English towns, including London, the birth rate averaged 28.2 per 1,000; in London the rate was 26.8. During the third quarter of the year the deaths of 120,792 persons were registered, equal to an annual rate of 14.0 per 1,000; this rate was 2.5 per 1,000 below the mean rate in the corresponding periods of the ten preceding years. The lowest county death rates last quarter were 10.3 in Surrey and in Hertfordshire, 10.5 in Berkshire, 10.6 in Bedfordshire, 10.7 in Dorsetshire, and 10.8 in Cambridgeshire; the highest rates were 15.1 in the East Riding of Yorkshire, 15.9 in Glamorganshire, 16.1 in Northumberland and in Monmouth, 16.8 in Lancashire, and 17.1 in Durham. In seventy-six of the largest English towns, with an aggregate population of more than fifteen and a half millions, the mean rate of mortality was 15.2 per 1,000; in 141 smaller towns, containing in the aggregate about four and three quarter millions of persons, the rate averaged 13.3 per 1,000; while in the remaining, and chiefly rural parts of England and Wales the rate was 13.0 per 1,000. In London the death rate was 14.6 per 1,000. The rate of infant mortality, measured by the proportion of deaths among children under 1 year of age to registered births, was equal to 155 per 1,000, against an average rate of 192. In the several counties the rates of infant mortality ranged from 61 in Cambridgeshire, 64 in Dorsetshire, 68 in Somersetshire, 70 in Buckinghamshire, 75 in Hertfordshire, and 76 in Shropshire, to 176 in Glamorganshire, 182 in the West Riding of Yorkshire, 186 in Durham, and 200 in Lancashire and in the East Riding of Yorkshire. In the seventy-six large towns the mean rate was 186 per 1,000; in London the proportion was equal to 174 per 1,000, while it averaged 191 in the seventy-five large provincial towns, among which the rates ranged from 74 in Hornsey, 75 in Bournemouth, 77 in King's Norton, 88 in Burton-on-Trent, and 89 in Hastings, to 261 in Sheffield, 267 in Wigan, 275 in Norwich, 277 in Stockport, 282 in Rhondda, and 354 in Grimsby.

UNIVERSITY AND EDUCATIONAL NEWS.

By the will of Joseph E. Gillingham, numerous bequests are made to educational and charitable institutions, including $50,000 each to the University of Pennsylvania for the veterinary department, to Haverford College, to Swarthmore College and to Bryn Mawr College.

The trustees of the agricultural school, provided for in the will of Oliver Smith, of Hatfield, sixty years ago, have bought a site in Northampton, Mass., in order to be ready for establishing the school when the funds become available on December 22. The purchase consists of ninety-three acres, obtained for $19,450. The endowment now amounts to $312,000.

The school fund of Minnesota now amounts to over $16,000,000, and is increasing at the rate of a million dollars a year. It is said that this fund, which by the constitution of the state accrues from the sale of certain lands, will ultimately amount to $100,000,000, only the interest of which can be used for school purposes.

By the will of the late Mr. J. E. Williams, the University of Wales receives £10,000 and the University College of North Wales at Bangor £12,000.

The council of University College, Dundee, have declined Mr. J. K. Caird's gift of £16,000 for the erection of a physical laboratory on

the college grounds, judging Mr. Caird's conditions to harmonize neither with the needs of the college nor with the general plan of building construction.

The tenth annual meeting of the National Association of State Universities began at Washington, D. C., on November 13, with thirty-two presidents of state universities present. The session was devoted to the annual address of Chancellor E. Benjamin Andrews, of the University of Nebraska, president of the association, and to a discussion of the attitude state universities should take toward graduate work.

The new chemistry building of the University of Wisconsin, which has been in the course of construction during the past year, has been completed and is now ready for occupancy. The building is completely equipped with apparatus for the study of all branches of the subject, including electrical, physical and pharmaceutical chemistry. The auditorium on the main floor will accommodate between 500 and 600 students, and the laboratory for general chemistry which adjoins it has 540 individual working desks. Smaller research laboratories for graduate and advanced students, a laboratory for inorganic chemistry, and the offices of the professors of the department, occupy the remainder of the first floor. Two large laboratories for analytical chemistry, the department library and two lecture rooms constitute the second floor. On the third floor are placed the laboratories for pharmaceutical, physical and electrical chemistry, and the office and laboratory of the state chemist. The old chemical laboratory on the shore of Lake Mendota has been remodeled for the departments of chemical engineering and assaying. The quarters in North Hall formerly occupied by the department of pharmacy have been remodeled and are now occupied by the department of commerce. Besides lecture and class rooms, a portion of the hall has been set aside for the new commercial museum, which is rapidly being arranged for the use of the commerce students.

The number of students registered this term at Cambridge University is 1,008, distributed as follows: King's, 56; Trinity, 200; St. John's, 66; Peterhouse, 12; Clare, 61; Pembroke, 76; Gonville and Caius, 86; Trinity Hall, 44; Corpus Christi, 21; Queen's, 10; St. Catharine's, 15; Jesus, 57; Christ's 46; Magdalene, 15; Emmanuel, 76; Sidney Sussex, 27; Downing, 32; Selwyn Hostel, 39; non-collegiate, 39.

Dr. Herbert Moody has been appointed assistant professor in analytic chemistry at the College of the City of New York. He is a graduate of the Massachusetts Institute of Technology, and for four years was instructor there.

The committee for the supervision of instruction in geography at Oxford has appointed Dr. A. J. Herbertson, director of the school of geography, for the remainder of the term of five years for which the grants to the school of geography have been voted.

At Manchester University, Mr. C. G. Hewitt, B.Sc. (Man.), has been appointed assistant lecturer and demonstrator in zoology; and Mr. A. Stephenson, B.Sc. (Wales), lecturer in the Technical College, Sunderland, assistant lecturer in mathematics.

The council of King's College has made the following appointments: Mr. E. P. Harrison, Ph.D., and Mr. H. S. Allen, M.A., B.Sc., assistant lecturers in physics; Mr. C. F. Russell, B.A., assistant lecturer in mathematics; Mr. L. Hinkel, assistant demonstrator in chemistry; Mr. W. Woodland, demonstrator in zoology; Mr. O. S. Sinnatt, B.Sc., and Mr. R. Wolfenden, B.Sc., demonstrators in engineering; Mr. J. E. S. Frazer, F.R.C.S., transferred from St. George's Hospital to King's College as demonstrator in anatomy.

Dr. René du Bois-Reymond has been appointed head of the department for special physiology in the physiological laboratory of the University of Berlin, in succession to the late Professor P. Schultz.

Dr. G. Hellmann, chief of department in the Royal Meteorological Institute in Berlin, has been appointed professor in the university. A department library and reading room has also been provided.